STUDIES

FROM THE

PSYCHOLOGICAL LABORATORY

UNIVERSITY OF WISCONSIN

1889-1893.

JASTROW.

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FROM THE LABORATORY OF EXPERIMENTAL PSYCHOLOGY

OF THE

UNIVERSITY OF WISCONSIN.

Series First, Second, Third, and Fourth.

1889 to 1893.

From the American Journal of Psychology.

Joseph Jastrow, Professor of Experimental and Comparative Psychology, and Director of the Laboratory of Experimental Psychology.

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MINOR CONTRIBUTIONS.

STUDIES FROM THE LABORATORY OF EXPERIMENTAL PSYCHOLOGY OF THE UNIVERSITY OF WISCONSIN.

BY JOSEPH JASTROW, PH. D.

By way of introduction to the first appearance in print of this Laboratory a few words may not be out of place. The Laboratory was founded in connection with the chair of Experimental and Comparative Psychology, established in the fall of 1888, the duties of which, I at that time assumed. The object of the Laboratory is primarily to give opportunity of demonstrating the chief points in a course in psychology, and of allowing students to test for themselves the simpler results of the methods of observation and experiment, and secondly to provide facilities for advanced and original work. In the programme of an American college the former end must stand out more prominently than it would, for example, in a German university. The original work in turn must be more directly under the guidance and control of the director of the Laboratory and the themes suited to the capacities and available time of the student. These, as well as the necessarily slow growth of a somewhat novel department, form the chief,—but I hope and believe constantly decreasing difficulties in the way of giving the actual a reasonable approximation to the ideal. My policy, however, is not to bring the researches conducted in the Laboratory under any one general scheme, but allow them to be suggested by the interest of the student, by the facilities of the Laboratory, or by the fluctuations of interest in the psychological world.

Regarding the present contributions, I have only to say that they will give evidence of some of the limitations under which they were worked out, but that I thought it wise not to delay their publication in anticipation of future results, but to send them forth as they are, to excite whatever interest and encourage whatever research they may. They are directed mostly to points connected with the problems of the psychophysic law, and may, perhaps, contribute a little toward bringing a much desired unity of conception into that vexed field.

Under the appropriate heading are mentioned the names of

the students who obtained the experimental results either directly with me or upon one another under my guidance. I trust to be able to continue these contributions at about yearly intervals.

On the Psychophysic Series.

The conformity of the magnitudes of the stars to the series demonstrated by the psychophysic law still remains one of the most striking applications of this law as well as an important piece of evidence in its favor. The stars were arranged in magnitudes on the basis of their naked-eye appearances, and at a time when any objective determination of their brightness was impossible. It is natural to suppose that the astronomers had in mind a sort of series in which the average stars of each magnitude should be separated by equal differences of brightness; i. e., by equal differences of sensation. When, now, we come to compare this psychic series with the physical series formed by the photometric determinations of the average stars of the several magnitudes, we find that this latter is approximately a geometric series with an average ratio of 2.5, for the first five or six magnitudes. To the arithmetical series of sensations separated by equal sensory differences there corresponds a geometric series of stimuli separated by a constant ratio; and this is the relation most closely answering to Fechner's formulation of his law. It is the most direct method of testing whether the sensations increase in arithmetic ratio as the stimuli increase in geometric ratio; i. e., whether the sensation increases with the logarithm of the stimulus. In this Journal, Vol. I, pp. 112–127, I have traced in detail the agreement of the estimations of star magnitudes with the phychophysic law, and in the present study my aim is to test whether this method can be applied to other fields of sensation, (for this, to my knowledge, has not yet been done), and with what results.

A.—Visual Extension.

My first attempt was with spacial relations of vision. A very large number of thin sticks varying arbitrarily in length from a few millimetres up to about 300 millimetres were mixed together in a random order; and the problem of the subject was to arrange these sticks according to length in a given number of classes or, to keep the comparison, of magnitudes. For this purpose I had made a frame with nine square openings, each one foot square, and with a bag hung within each compartment.[1] The whole was conveniently supported so that a person could sit with the sticks next to him

[1] The apparatus was constructed from a grant made me by the Elizabeth Thompson Science Fund, which I again gratefully acknowledge.

and sort them out according to a general impression of size. But one stick at a time was seen, and as soon as it was thrown into the bag it was lost from the subject's view. At first one's idea of the average length of each magnitude is vague, being founded only on the lengths of the extreme sticks that had been shown at the beginning of the experiment; but as one goes on his idea soon becomes clear, though puzzling cases of sticks just on the boundary between two magnitudes will always occur. When several hundred sticks had been thus assorted they were taken out and measured and the average length of the sticks in each bag computed. If the psychophysic law holds true of sensations of visual extension when thus tested, then these averages should form a geometric series with a constant ratio, just as do the photometric determinations of the average star-magnitudes. My results include the records of five persons sorting the sticks into six divisions, and of five sorting them into nine. Regarding the former, one observer declared himself dissatisfied with the result owing to the changing of the standards during the operation so that too many sticks had been thrown into the "longest" compartment. On examination this was found true and I have therefore omitted his result; the omission, however, does not effect the average result. The other four results are:

I.	Number of sticks,	79	133	89	70	56	88
	Average length,	18.5	55.6	97.7	146.8	194.7	251.1
II.	Number of sticks,	122	137	113	57	61	25
	Average length,	25.1	61.8	124.6	195.6	239.0	278.9
III.	Number of sticks,	236	65	59	59	60	37
	Average length,	45.9	106.1	147.5	184.2	231.3	273.8
IV.	Number of sticks,	200	79	103	52	51	50
	Average length,	36.8	90.1	142.3	200.7	239.9	275.2
Ar.	Number of sticks,	159	104	91	60	57	50
	Average length,	31.6	78.4	128.0	181.8	226.2	269.8

The last lines of figures represent the averages of I, II, III and IV. The following is a similar result for the sorting into nine magnitudes by five other observers, and their average:

I.	Number,	116	87	70	43	51	42	29	40	27
	Av. length,	62.8	84.5	111.7	139.5	164.5	187.8	215.5	233.5	252.2
II.	Number,	57	63	35	47	53	57	81	51	63
	Av. length,	44.5	66.6	73.5	92.0	107.4	136.2	164.9	212.8	231.8
III.	Number,	36	85	83	86	71	44	37	41	21
	Av. length,	40.3	65.4	86.6	121.6	156.6	190.3	218.2	235.0	254.8
IV.	Number,	15	27	39	67	86	116	78	69	11
	Av. length,	33.6	41.9	57.6	71.4	95.1	139.0	186.3	235.4	256.3
V.	Number,	56	80	82	63	66	58	56	27	18
	Av. length,	43.2	67.7	91.5	119.1	151.0	177.3	221.0	240.4	256.3
Ar.	Number,	56	68	62	61	65	63	56	46	28
	Av. length,	44.9	65.2	84.2	108.7	134.9	166.1	201.2	231.4	250.3

We need only compare the successive differences of the several magnitudes with their successive ratios to obtain an answer to our problem. Doing this for the average result we have:

Average difference,	46.8	49.6	53.8	44.4	43.6
Average ratio,	2.48	1.63	1.42	1.24	1.19

Average difference,	20.3	19.0	23.5	26.2	31.2	35.1	29.2	18.9
Average ratio,	1.45	1.29	1.27	1.24	1.23	1.21	1.15	1.08

In the division into six magnitudes it is quite clear that we have to deal with an *arithmetical* and not a geometrical series, or that the result is quite different from the result with star-magnitudes. In the division into nine magnitudes the difference between the two series is naturally considerably less, and so a decisive result the more improbable. The averages are considerably more irregular,[1] and the process is in every way more difficult. But if we regard the individual records as well as their average, we find that the balance of evidence tends towards making this also a coarsely approximate arithmetical series. If the series tends to a geometric one, it would be indicated by a tendency of the differences to rise with the magnitudes. Judged by this, test Numbers I, III and V, in the last table, are more or less arithmetical in their tendencies; Number II is very irregular, but can hardly be said to favor the geometric series; while Number IV does distinctly lean to the geometric. By a fortunate chance, Number IV is the only subject who appears in both experiments being the Number I of the "six division" series; and if we refer to that record, we find a very similar tendency there, though in the average it is entirely overbalanced by the "arithmetical" tendencies of the other three observers. We have thus indicated that whether or not the psychophysic law is obeyed in these experiments may be an individual matter. As a further test of this relation, I asked all of those who sorted the sticks into six magnitudes (as well as some of the others), after they had finished, to draw six lines of the lengths, equal to the average sizes of the magnitudes

[1] I shall not discuss the nature of these irregularities further than to emphasize the importance of the *number* of sticks in each magnitude upon the average length; the numbers are irregularly distributed, and it is very noticeable that so frequently when the number of sticks is very much larger, or very much smaller, than the average number, the average *length* of these sticks also deviates from the usual average. Again, the first and the last magnitudes are apt to be irregular, because all very small sticks go into the one, and all very large ones into the other, and the number of such sticks present will evidently affect these averages. When a large number of sticks is placed in the smallest magnitude, its average will be high, and the reverse is true for the highest magnitude. A similar effect is noticeable in star-magnitudes, for which see my paper as above cited.

which they had in mind when sorting the sticks. These estimations agree as well as could be expected with the results of measurements, both in the average (which I here append)

Lengths of lines:	47.5	82.5	120.0	156.7	193.5	244.7

and in the individual records, the subject with the distinct "geometric" tendencies also revealing this trait in the lines he drew. This would indicate a rather more definite and conscious representation of the several standard magnitudes than I for one should have anticipated.

To express the degree of approach of the average results in the two sets of experiments to an arithmetical series, I append these averages, together with the ideal series, to which they most closely approximate:

Real Series,	31.6	78.4	128.0	181.8	226.2	269.8
Ideal Series,	32.1	80.3	128.5	176.7	224.9	273.1

Real Series,	44.9	65.2	84.2	108.7	134.9	166.1	201.2	231.4	250.3
Ideal Series,	35.4	62.3	89.2	116.1	143.0	169.9	196.8	223.7	250.6

We can further express the average deviation of the actual from the ideal series as a percentage of the average lengths, and will find this to be 1.6% for the first set, and 3.8% for the second. These figures may be regarded as measuring the approximation of the result to an arithmetical series.

B.—Tactual-Motor Extension.

With the assistance of Lucien Mason Hanks and James Bremer Kerr.

The above mentioned experiments were made at the Psychophysical Laboratory of Johns Hopkins University, in the spring of 1888. In order to extend the application of the method, and to investigate whether the result would be the same with a less accurate sense, I decided to continue the study at my present laboratory by performing the same operation of sorting the sticks into six magnitudes, but with the difference that the sticks were not *seen* by the subject. The latter simply *felt* their lengths by moving his forefinger along them and announcing the compartments in which he wished them placed. Each was then thrown into the bag by an assistant, who also gave the subject the next stick he was to feel. The process is thus the same, except that this form of tactual-motor sensation takes the place of visual sensation. The test was made with four subjects. The range of sticks in length was a little narrower than with visual judgments (the longest stick being about 25 mm. shorter than the longest stick with visual judgments), and the number of sticks also smaller—about 360 against 500. The number of sticks and

their average length for each observer, and their average is as follows :—

I.	Number,	60	69	79	50	50	53
	Average length,	35.2	71.3	112.8	158.2	189.9	235.1
II.	Number,	67	81	69	68	48	27
	Average length,	57.0	76.1	118.6	170.6	206.4	244.3
III.	Number,	55	64	69	66	50	54
	Average length,	35.1	67.5	100.6	148.6	190.6	238.6
IV.	Number,	60	35	56	57	52	97
	Average length,	37.1	63.2	85.9	117.3	162.6	224.0
Av.	Number,	60	62	68	60	50	58
	Average length,	36.1	69.5	104.5	148.7	187.4	235.5

The ideal series, to which the average of the four results approximates, is 40.55, 70.45, 110.35, 150.25, 190.15, 230.05, the average deviation of the two series expressed as a percentage of the average length being 2.6%. With regard to the individual records nothing requires special mention, except the fact that Number IV shows a tendency to follow the geometric series, especially so if we take into account the error in the average length of the lowest magnitude due to its being the lowest. In brief, the result is in every respect essentially similar to that with visual magnitudes, and all that has been said of the latter applies with equal force to the former.

The nature of the result being thus clear, I will at the present time offer nothing more than a few thoughts in explanation of the holding good of the law with star-magnitudes and its failure with extension magnitudes. The two queries that these results suggest are: With regard to what class of sensations can the psychophysic law be expected to hold good? And may the agreement with the law depend upon the method by which it is tested? Respecting the former it seems to me that the law includes such sensations as are appreciated *en masse*, and with not too distinct a consciousness of their intensity; when the sensation is a sort of impressionist reception of the gross sensation without dividing it up into units, or conceiving it as so composed, we may expect the law to hold good. This would be the case with the rough estimations of star brightnesses. On the other hand, when the impression is consciously received and definite in extent, as with spacial relations, the correspondence of the arithmetical with a geometrical series can not be expected, for if I am asked to draw a series of equally different lines, or if I am asked to sort sticks into groups, I have in mind the division of the range into *equal* groups, and I cannot help asking myself whether these groups are to be equally different *absolutely* or

relatively. The former seems to be the simpler and more natural conception, and it is accordingly adopted, whenever the problem becomes a conscious one; that this is what the subject has in mind, is clear from the lines he draws as the equivalents of his average magnitudes. Again, the individual who follows the geometric series would be one who did not consciously state the problem to himself, but went on a general impressionist view of the matter. At present this is offered merely as a suggestion that brings harmony into the results and emphasises the important part played by consciousness in the estimation of sensations. With regard to the second question I desire only to bring it into relation with the first, by calling attention to the fact that the psychophysic law seems to hold good of this class of extensive sensations when tested by other methods, and that therefore possibly a difference in the mental attitude of the subject may decide whether the sensation will be perceived under the psychophysic law or not. Apart from the interest in the experiments as an extension of a psychophysic method to new fields, these are the points of view from which I trust the present research may be of interest.

The Perception of Space by Disparate Senses.

With the assistance of Frederick Whitton.

In a paper under this title, published in *Mind*, XI., No. 44, I offered the following as a provisional, but perhaps convenient, classification of the avenues by which we could gain knowledge of spacial relations:—

"I. By the stimulation of a definite portion of a sensitive surface:

(1) Of the retina (where the distance of the stimulating object must be inferred.)

(2) Of the skin.

(*a*) By the application of a pair of points, leaving the intermediate skin unstimulated, or

(*á*) Stimulating it by the application of a straight edge.

(*b*) By the motion of a point along the skin (see *Mind*, 40, pp. 557.)

[(*a*) and (*b*) may be contrasted as simultaneous and successive.]

"II. By the perception of distance between two movable parts of the body, *e. g.*, between thumb and forefinger.

"III. By the free motion of a limb, *e. g.*, the arm."

I then proceeded to investigate in detail the space relations furnished by a variety of I (1), of II and of III, deducing a

series of relations in part to be referred to in the present study, but for a full account of which the reader must have recourse to the original memoir. In the present study a form of I (2) (*a*) was interpreted by drawing lines with the aid of the eye, in which process the eye is naturally the guiding sense.

The method of work was as follows: Two spots were marked upon the volar surface of the forearm of each arm, one near the elbow, the other near the wrist. One of a pair of points was applied either to the lower (near the wrist), or the upper (near the elbow) of these points, and the other at various arbitrarily selected distances from the former. It goes without saying that the subject was prevented from seeing the pair of points applied to the arm by the interposition of a screen. Ten observations were made in a set, keeping one of the points constant throughout. The subject appreciated the distance between the points, and drew with a pencil a line, the length of which seemed to his tactual sensation, (not to his judgment or actual knowledge of the relation,) equal to it.

Even this was a difficult task, owing to the coarse sensibility of the forearm, and the estimations were made with little confidence and much hesitation and fluctuation. Owing to this, it was allowed to have the points applied (for a moment only) as often as the subject required, and he could correct and recorrect the lines drawn, until he felt satisfied with the result. Again, the arm fatigues very easily, especially at and near the point under constant stimulation, this being mainly due to the rather strong impression of the points necessary to give a distinct sensation. The apparatus employed was the æsthesiometer I described and figured in the proceedings of the American Association for the Advancement of Science, 1887, and also partly in this JOURNAL, Vol. I, p. 552.[1] I again take the opportunity of gratefully acknowledging the grant made me by the "Elizabeth Thompson Science Fund," by aid of which this apparatus was constructed.

The smallest lengths applied were determined by the smallest distance between the two points still felt as two; the largest by the dimensions of the forearm. Four cases were distinguished, according as (1) the right or (2) the left arm was

[1] The only change made was in setting the bar bearing the points upon adjustable brackets projecting at right angles from the uprights, to enable the arm to rest more conveniently beneath it. I will not describe the apparatus further than to remark that it offered great facility in leaving the operator both hands free for work, in applying both points equally well and always in the same way, and in making the setting and recording of distances extremely easy. The only difficulty is in the disposition of the arm to give both ease of application and comfort.

used, and as (*a*) the upper or (*b*) the lower point was kept constant, the latter distinction is necessary, because the sensibility differs at the two points. This was tested as a rule both before and after each set of ten observations; it being found that the fatigue incident to the experiments diminished the sensibility. The results of these observations are embodied in the following table:

J. JASTROW.	RIGHT ARM.		LEFT ARM.	
	UPPER CONSTANT.	LOWER CONSTANT.	UPPER CONSTANT.	LOWER CONSTANT.
Before	58.0	31.7	57.4	33.0
After	68.8	46.0	73.2	42.5
Average	63.4	38.9	65.3	37.8
F. WHITTON.	**RIGHT ARM.**		**LEFT ARM.**	
	UPPER CONSTANT.	LOWER CONSTANT.	UPPER CONSTANT.	LOWER CONSTANT.
Before	52.3	32.2	64.8	37.2
After	64.0	41.7	77.0	51.0
Average	58.2	37.0	70.9	44.1

The numbers express in millimetres the distances between two points just felt as two. It would be fairest to consider the average sensibility throughout the experiments as the mean of the sensibility before and after, and this is accordingly added in the table. The table shows: (1) That the sensibility at the lower point (near the wrist) is finer on both arms and for both observers than at the upper point (near the elbow), and on the average the points are perceived as distinct when 25 mm. nearer. (2) That the average just perceptible distance is for the upper point 64.5 mm., for the lower 39.5 mm. (3) That for Mr. Whitton the right arm is more sensitive both above and below than the left, while no such difference is apparent for myself. (4) That the effect of the fatigue increases the just perceptible difference after ten observations on the average by 12.2 mm.

As regards the chief object of the investigation, I have in the following table divided the observations into five groups, aiming to have the averages of the groups separated by about equal intervals, and have placed under each average distance between the points, as applied upon the forearm, the average length of the lines by which it was represented, and under this in turn the ratio of the two expressed as a percentage. This is done separately for Mr. Whitton and myself, and with the distinction of the four cases as already noted.

J. Jastrow.

	RIGHT ARM: UPPER POINT CONSTANT. 1.	2.	3.	4.	5.	RIGHT ARM: LOWER POINT CONSTANT. 1.	2.	3.	4.	5.
Real length . . .	64.0	80.0	101.2	120.0	139.5	59.4	79.0	101.7	121.9	145.0
Drawn length . .	15.0	19.7	25.2	38.1	56.4	17.6	23.0	32.7	42.9	53.8
Ratio in percentage	23.4	24.6	24.8	31.7	40.4	29.6	28.1	32.1	35.2	37.1

	LEFT ARM: UPPER POINT CONSTANT. 1.	2.	3.	4.	5.	LEFT ARM: LOWER POINT CONSTANT. 1.	2.	3.	4.	5.
Real length . . .	63.0	80.4	102.3	123.0	138.0	60.4	81.3	100.8	121.5	141.0
Drawn length . .	12.7	17.8	29.8	43.7	56.1	14.9	23.0	32.1	41.9	61.4
Ratio in percentage	20.2	22.1	29.1	35.5	40.6	24.7	28.3	31.8	34.5	43.6

F. Whitton.

	RIGHT ARM: UPPER POINT CONSTANT. 1.	2.	3.	4.	5.	RIGHT ARM: LOWER POINT CONSTANT. 1.	2.	3.	4.	5.
Real length	64.2	82.7	101.2	120.8	138.3	55.6	79.2	99.8	120 0	137.5
Drawn length. . .	42.8	51.1	70.6	79.2	87.7	39.3	58.2	74.8	82.1	98.8
Ratio in percentage	66.7	61.8	69.8	65.6	63.5	70.7	73.5	75.0	68.4	71.9

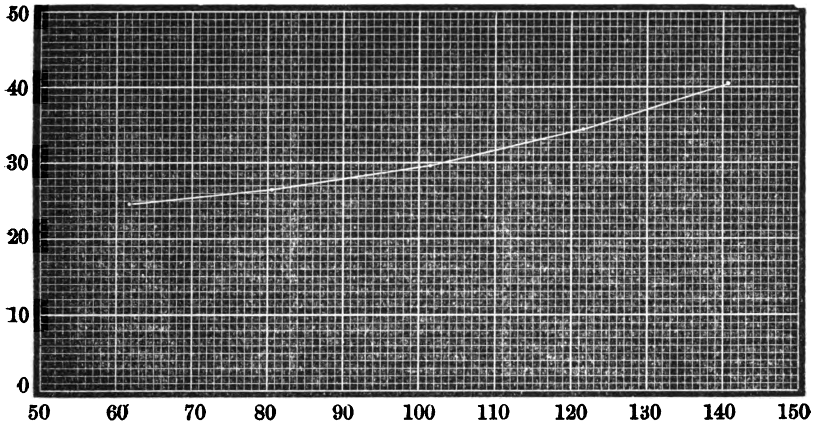
	LEFT ARM: UPPER POINT CONSTANT. 1.	2.	3.	4.	5.	LEFT ARM: LOWER POINT CONSTANT. 1.	2.	3.	4.	5.
Real length. . . .	65.0	82.0	102.5	121 7	141.2	57.7	80.3	100.7	117.5	129.6
Drawn length. . .	30.0	42.5	60.5	75.8	97.0	39.7	59.9	70.0	88.5	97.6
Ratio in percentage	42.2	52.0	59.0	62.3	67.2	68.8	74.6	69.6	75.3	75.3

These Tables show: (1) *That the lengths are all very much underestimated*, the lines being on the average 65.6% of the distances between the points for Mr. Whitton and but 30.9% for myself; (2) That for myself throughout *the underestimation decreases as the length increases*, though for Mr. Whitton this is true in one of the four cases only; (3) That the underestimations are less when the lower point than when the upper point is constant, on the average by 7.2 mm. for Mr. Whitton, by 3.9 mm. for myself; (4) That for myself there is no difference between the sensibility of the two arms, but for Mr. Whitton the right arm is slightly more sensitive than the left.

Postponing the further discussion of these results, I will assume that the average sensibility along the arm is that midway between the sensibility at the upper and at the lower point, and that there is no difference in sensibility between the two arms; I then take my own result as rather the more regular of the two and obtain the most typical result by combining the four cases for myself as is done in the following table:

Real length,	61.7	80.2	101.5	121.6	140.9
Drawn length,	15.1	20.9	29.9	41.7	56.9
Ratio in per cent.,	24.4	26.1	29.5	34.2	40.4

The same is expressed graphically in the accompanying curve.[1]



I shall now discuss the relation of this result to the conclusions of my former paper. Such discussion can only include the most general relations, a minute comparison being impossible owing to the difference in the number of subjects and observations. The most general conclusion of my former paper here pertinent is that "*If the eye is the expressing sense all lengths are greatly underestimated*, the error decreasing as the length increases.[2] With this general result this curve is entirely in agreement, although the decrease of the error with the increase of the length is not as marked, owing in part to the smaller range of lengths that the observations cover. Regarding the comparative accuracy of the feeling of tension between thumb and forefinger, the motor sensations of the arm and the skin sensibility of the fore-arm, accurate statement is impossible, but the indication is that the last is a less accurate source of space-perception than either of the others. My general result is thus an additional verification of the conclusions reached in my former study, and an extention of their significance. The space-perceptions of disparate senses are themselves disparate, and whatever harmony there is among them we are warranted in regarding as the result of experience. The

[1] The ordinates express the drawn lengths as percentages of the real lengths, the latter being indicated by the abscissæ.

[2] Though the method of expressing by the eye is different here from what it was in the former study (no pains being taken to restrict the movement of the eye and the hand moving over the space drawn). I do not think it likely that this difference at all seriously influenced the results owing to the supremacy of the eye in all spacial judgments.

spacial notions of one deprived of the sense of sight and reduced to the use of the other space-senses must indeed be different from our own. And the existence of the striking disparities between our visual and our other space-perceptions, without confusing us, and, indeed, without usually being noticed, can only be explained by the tendency to interpret all dimensions into their visual equivalents and unconsciously correct them by the same means. The general law to which the result contributes seems to establish a sort of co-efficient of conversion; the same amount of objective stimulation upon a delicately sensitive surface is interpreted subjectively as the equivalent of a much more extensive sensation than an equal objective stimulation upon a coarsely sensitive sense-organ. There is, as it were, an exchange of the spacial units of different senses, and because the visual units are the smallest it takes a smaller visual space to seem equal to a larger tactual or motor space.

A few points peculiar to the present research remain to be noted. The first is the peculiar fact that when the points are extended a few millimetres there is a sort of jump from the point at which no interval at all is felt (the two points being felt as one) to the perception of the entire interval. The moment we perceive an interval at all, we regard it as longer than the mere separation of two points; it is not that the zero point is at a constant height, but that the sensation changes its character. To my knowledge the theory of dermal sensibility is too little advanced to give an adequate explanation of the fact, nor have I any to offer. The fact itself seems to me important, and must be accounted for by any theory that claims general acceptance. A second point is that while the sensibility at the upper and at the lower points differs by about 25 mm. the difference in their reproductions is only about 5.5 mm. Even if we regard this difference as subject to the same underestimations as the absolute lengths it is strikingly small; but the explanation of the fact is even more difficult than of the foregoing.

On the Pressure Sense.

With the assistance of Sarah Belle Flesh and Helen Smith.

The problem set proved a much broader one than could be profitably worked out in the limited time at the disposal of the experimenters, so that only two aspects of the work can be here described, both of these relative to the methods of testing sensibility. The apparatus used for testing the pressure-sense was a modification of a Fairbanks' post-office balance, in which the initial and incremental weights were

placed upon the scale-pan, thus exerting an upward pressure upon the finger situated at the end of the beam. A series of attachments were added by which the pressure could be instantly released from the finger and thus the ill effects of fatigue averted. A comfortable and firm position of the arm, hand and finger was also secured. To obtain a normal sensibility, experiments were made according to the method of right and wrong cases, the subject being requested to answer each time, and doubtful answers being excluded so that half the answers would be correct by chance. At the bidding of the subject a pressure was brought to bear upon the finger; at a second signal the pressure was increased or diminished, and at a third the original weight was restored. The subject had to decide whether the middle pressure was lighter or heavier than the extremes. The two initial weights applied were (A) 315 and (B) 105 grms., and the changes were an increase or decrease by (1) $\frac{1}{7}$ or (2) $\frac{1}{21}$ of these weights. An attempt was also made to record the confidence[1] in the correctness of one's answer on a scale in which 3 signified relative certainty, 0 no preference for one answer above its opposite, and 1 and 2 intermediate grades of feeling. After throwing out certain observations made under distracting circumstances there remain 100 observations for each observer under each of the four cases. These are given in the table, together with the theoretical ratio at which according to the formula given in my paper published in this JOURNAL (Vol. I, p. 308), one-fourth of the answers should be correct.

MISS SMITH.

Initial weight.	Ratio of increment.	Percentage of error.	Ratio at which 25 per cent. errors would occur.	Average confidence
315 grammes	$\frac{1}{7} = 1.143$	4.0	1.053	1.22
315 "	$\frac{1}{21} = 1.048$	19.0	1.037	0.60
105 "	$\frac{1}{7} = 1.143$	3.0	1.049	1.17
105 "	$\frac{1}{21} = 1.048$	20.0	1.038	0.60

MISS FLESH.

Initial weight.	Ratio of increment.	Percentage of error.	Ratio at which 25 per cent. errors would occur.	Average confidence
315 grammes	$\frac{1}{7} = 1.143$	10.0	1.073	0.78
315 "	$\frac{1}{21} = 1.048$	34.0	1.080	0.54
105 "	$\frac{1}{7} = 1.143$	12.0	1.077	1.14
105 "	$\frac{1}{21} = 1.048$	40.0	1.132	0.62

The constancy of these numbers measures the constancy of

[1] This method was used in the research by Mr. Peirce and myself on "Small Differences of Sensation," Memoirs of the National Academy, Vol. III, and also in the paper in *Mind*, No. 44.

the sensibility as well as the agreement of the results with the requirements of the psychophysic law. The law seems approximately adhered to, though with variations depending largely on the small number of observations. The average ratio at which 25 per cent. of errors should occur is for Miss Smith 1.044, for Miss Flesh 1.090, the mean of which is 1.067; and as this measures the most probable error we in a certain sense express the fineness of the pressure sense as here determined, by saying that its probable error is 1.067 or about $\frac{1}{15}$.

A second series of observations was made under the same conditions except that instead of applying and removing the additional weight while the initial weight is upon the finger, the initial weight is applied and removed; then the initial plus or minus the additional weight is applied and removed; and then the initial alone again. The question is whether we can compare more accurately the change of a sensation x with the sensation $x \pm a$ (produced by simply adding or subtracting a), or the entire sensation x with the entire sensation $x \pm a$. The result for Miss Flesh is too much affected by what must be accidental errors to be here cited, but for Miss Smith it is as follows; the result is arranged as in the preceeding:

Initial weight.	Ratio of increment.	Percentage of error.	Ratio at which 25 per cent. errors occur.	Average confidence.
315 grammes	$\frac{1}{7} = 1.143$	11.2	1.077	0.27
315 "	$\frac{1}{21} = 1.048$	30.0	1.062	0.11
105 "	$\frac{1}{7} = 1.143$	12.5	1.081	0.27
105 "	$\frac{1}{21} = 1.048$	31.6	1.068	0.00

We see that this second method is decidedly the more difficult, the average "probable error" rising, for Miss Smith, from .044 to .072. The psychophysic law is well supported, though here as before the subject appreciates differences of $\frac{1}{21}$ relatively better than differences of $\frac{1}{7}$. Regarding the causes of the increased difficulty of the second method of experimentation it may be in point to note that memory has a wider play in it than in the former method, though this is not the entire psychological difference. The result shows too, how essentially tests of sensibility are dependent upon the methods employed.

Regarding the confidence we see that it rises as the proportion of error decreases and falls as this proportion increases; what this relation is I have no means of determining, nor do I think that it is constant or anything more than a subjective but practically useful aid in judging the reliability of the results.

On Just Observable Differences.

With the assistance of Augusta Adrienne Lee.

The usual applications of the method of the Just Observable Difference aim to fix by more or less direct means the point at which two sensations are sufficiently different to have that difference consciously perceived when the attention is directed to it, and to arouse some confidence in the correctness of one's judgment of this difference. I have elsewhere[1] pointed out the uses and the abuses of this method and will here confine myself to the description of a hitherto unnoticed mode of testing the Just Observable Difference. A distinction, the importance of which is not always recognized, is that between the power to tell that two stimuli are different and the power to tell the direction of this difference. In some cases the later is always given with the former, but in others it is not. A great many persons can tell that tones are different without being able to tell which is higher and which lower. It matters much, too, whether the two stimuli are successive or simultaneous; and in the estimation of spacial relations it is important whether the two stimuli are placed side by side, so that their relations are manifest, or not. The form of the method now to be described is certainly a useful variation of it, and yet as far as I know has not been employed. It consists *in having the subject produce a stimulus just longer (more intense) or just shorter (less intense) than a given stimulus;* instead of judging differences presented to him he produces the smallest difference that he can. By this method a knowledge of the direction of the difference is made necessary.

In the first series of experiments fifty lines were drawn, their lengths varying in an arbitrary manner from about 25 to 150 mm.; and after viewing one line it was covered over, and the attempt made to draw a line just longer than the one seen. In the next series the attempt was made to draw the lines just shorter than the original lines; and in a third series (in order to eliminate a constant error, if there be any), the attempt was made to draw the lines just equal. In another set of experiments the same three processes were repeated, but the original line was kept in sight while the second was being drawn, though the two were kept at some distance apart so as not to make a fitting of the ends of the lines possible. The average number of millimetres by which a line differs from the original line under the three cases and when the original line was visible or not is given in the following table. I also give with this, the ratio of the average length of the

[1] See this Journal, Vol. I, pp. 273-277 and 299-302.

line to this just perceptible difference expressed as a percentage.

ORIGINAL LINE NOT SEEN.		
Just longer.	Just equal.	Just shorter.
2.17 mm. = 2.75 %	0.73 mm. = 0.92 % (Total error = 1.20 mm.)	2.50 mm. = 3.60 %
ORIGINAL LINE SEEN.		
2.56 mm. = 4.01 %	0.36 = 0.56 % (Total error = 2.04 mm.)	3.78 mm. = 4.97 %

The conclusions that I draw from these results are: (1) that the error when the two lines are seen is less than when not (the case when the lines are drawn equal is no exception if we count as we ought the absolute error positive and negative; these cancel one another in the latter case and so give an appearance of greater accuracy); (2) the just perceptible difference is greater in drawing the just shorter than the just longer lines; (3) the error in drawing lines equal is quite small, and its effect upon the other results not marked enough to appear in these few observations; (4) the just perceptible differences are considerably larger than those found with the more usual method. This last I would bring under the general law that our powers of execution fall short of our powers of discrimination. If the psychophysic law is true it would appear in this method in the fact that the just perceptible difference would bear a constant ratio to the length reproduced. If I divide the lines into short, medium and long lines, I get three just perceptible differences that are approximately constant ratios of the average lengths. I desire here mainly to call attention to this psychophysic method as a natural and easy method of obtaining a reliable quantitative result, and one easily comparable with the results of other methods.

SERIES SECOND.

FROM THE AMERICAN JOURNAL OF PSYCHOLOGY.

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STUDIES FROM THE LABORATORY OF EXPERIMENTAL PSYCHOLOGY OF THE UNIVERSITY OF WISCONSIN.

By Joseph Jastrow, Ph. D.

I.—The Effect of Foreknowledge upon Repetition-Times.

(With the assistance of Frederick Whitton.)

The experimental contributions to the study of the effect of foreknowledge upon the times of simple mental processes may be thus briefly summanized. In simple reactions the nature of the stimulus is of course foreknown, but the precise moment of its appearance and its intensity may be left indefinite. It has been found that the omission of a preparatory signal, or an irregular interval between signal and stimulus, as also are irregular variation between more or less intense stimuli, all lengthen the simple reaction-time. In that form of a distinction-time, in which one particular stimulus is to be reacted to but all others are passed without reaction, it is found that the larger the number of possible stimuli (and therefore the less definite the foreknowledge) the longer the reaction-time. In adaptive reactions, with the number of modes of reaction constant the time will be longer as each mode of reaction is connected successively with one, with two, with three or with more and indefinitely many stimuli; the stimuli may or may not be grouped in classes. In association-times Münsterberg has shown that the preceding of a question asking for a personal preference or judgment between a pair of objects, by the mention of a dozen or so of the class of objects to which the pair belongs, decidedly shortens the time of answer to the question, in one series from 947 σ to 676 σ. [1] This last form of experiment is extremely interesting; it seems to show that although we cannot begin to say, for example, whether we prefer peaches to pears, until we have heard the full question,—" apples, plums, cherries, peaches, grapes, oranges, pears, figs, lemons, dates, apricots, pine

[1] For a more detailed account of these points see Jastrow, Time-Relations of Mental Phenomena; pp. 15-17, 39-40, 50-51, etc.

faculty, functioning as matter, takes on the form of the external object, so in rational thought the human faculty, or passive reason, as matter, unites with the conceptual relations that are immanent in our percepts, as form, and it is these conceptual relations which Aristotle designates as the active reason. When the two unite, actual thought is the result. The strangeness of this distinction lies in the fact that Aristotle attributes the activity that manifests itself in thought to the content thought about. From this point of view it would seem less correct to say that we think thoughts than that thoughts think us. Aristotle says that the passive reason comes into existence with the body and perishes with it, and during life participates in its states. But the active reason has nothing to do with the life of the body; has no bodily organ; does not come into existence by procreation, but enters the body from without; and is therefore unaffected by the destruction of the body. The immortality of this part can have, however, little worth for the individual, for it possesses neither memory nor self-consciousness.

Though Aristotle's statements regarding the active reason may seem to mark a relapse into dualism, yet his psychology as a whole is distinctly monistic. He conceives the development of the soul as running parallel to that of the body, and his method is a biological-developmental one. He is a keen observer of mental phenomena as well as a profound metaphysician; he brings to bear upon psychology as much of anatomy and physiology as was known in his time; and he everywhere brings human into fruitful relations with animal psychology. Finally, he delivers psychology from the premature influence of ethics, recognizing that ethics depends upon psychology, not psychology upon ethics. It is these merits which make Aristotle the greatest psychologist of antiquity.

apples,—which do you prefer, peaches or pears?,"—yet the time needed for this decision is much shorter than when the introductory series of words is omitted.

The object of the present study was to test this point in a much more simple type of reaction, and with a variable number of possible stimuli. We selected for this purpose the repeating aloud of spoken words, the operator called a word and as quickly as possible the subject repeated it, all the words used being monosyllables. We found as the average of about 250 experiments with each of us that the time needed for doing this when the word might *be any word whatever*, for J. J. 269 σ for F. W. 267 σ. We formed lists of words as follows: (a) 100 very common verbs, signifying simple actions; (b) 50 common names of animals; (c) 20 proper names, such as John, Frank, Bess, Kate; (d) 20 letters (omitting b, d, m, n, v, w, as confusing in sound or polysyllabic); (e) 10 common French words; (f) the ten numbers, 'one,' 'two,' etc. to 'ten.' Only one list of each class of words was used, so that we became increasingly familiar with the lists. Before each set of 20 experiments the entire list of words from amongst which the words for repetition were to be selected, was read aloud. The following table shows for each of us the average time needed to repeat words under these circumstances. Each result expresses the average of from 240 to 300 experiments.

The conclusion thus corroborated is that *as the range of possible words decreases in extent, as the subject's expectation is more and more definite, the time needed to repeat the word becomes shorter.* It indicates the power and the utility of a general direction of the mind in the line of a more specific operation; the entrance into the general field of attention as preparatory to entering its fovea; the apperception that precedes preception, or in Galton's words, the entrance into the ante-chamber of consciousness to prepare the way for admission to the audience-chamber. We have here an adaptive reaction, each different word forming a distinct stimulus and the vocal manipulation necessary to repeat it a distinct form of reaction. It would seem that we could not get ready to repeat a word until we knew what the word is, and yet a knowledge of the possibilities of the case really aids our expectation and shortens even so simple a process as repetition. We perform the coarse adjustment before the stimulus appears, leaving only the time of the fine adjustment to be measured. There exists all degrees of definiteness and indefiniteness of expectation, of fore-knowledge, and an increase of definiteness to a certain limit brings about a shortening of the mental processes of recognition and adaptation.

While the results are too few and too variable to admit o any detailed treatment a few more special points may be pointed out as suggested though not as established. The extreme regularity of the results, the gradual decrease from repetitions of one of an indefinite number of words, to one of 100, of 50, of 20 and of 10, is doubtless accidental; the times for repeating one of 50, one of 100 or one of an indefinite number of words, for F.W. and of the latter two for J. J. are practically the same, and indicate a limit to the range of expectation. To expect one of 100 words seems scarcely a more definite attitude than to expect any word whatever. With F.W. this seems true of 50 words as well. It seems clear that it takes less time to repeat one of 20 words than one of 50 words, and least to repeat one of the ten numbers. We know the numbers so well as a class and as a series that expectation is here most definite. A French word on the other hand is relatively unfamiliar, and it takes longer to understand and repeat it. To obtain the time needed for the mental portions of the process alone, we substract the simple reaction-times from the repetition-times. How the former was obtained will be explained in a note.

Dr. Münsterberg has attempted to carry the distinction between the "motor" and the "sensory" form of reaction into complex types of reaction; indicating by the former a more special attention to the modes of reaction, by the latter a more special attention to the stimulus. Dr. Martius attempted to repeat the experiments in every detail but failed to obtain the distinction. We found it difficult to maintain this difference of attitude in repeating words, and the results (see the table above) show practically no difference in our cases between the two forms of reaction; the average of all the "motor" experiments was for J. J. 245 σ, for F. W. 249 σ, of the 'sensory' for J. J. 249 σ, for F. W. 251 σ. Even in the simple reaction the difference is slight; but in the ordinary reaction with a finger-key one of us shows the difference. J. J.'s simple reaction to a sound by closing a key with the finger is 136 σ for 'motor,' 162 σ for 'sensory'; F. W.'s 133 σ and 137 σ. While these results have probably only an individual significance, yet in our present incomplete knowledge of the true nature of the distinction between 'motor' and 'sensory' reactions, they may be worthy of record. *Note upon apparatus and method.* Our apparatus and method were gradually perfected during the course of the experiments (covering a period of eight months) and only such of our results were included in the averages given above as were obtained by the same method and seemed fairly comparable with one another. We began by attempting to speak the word and press the key

with the finger at the same moment, the subject also repeating the word and pressing the key as nearly as possible at the same moment. (We used keys of the form to be described in the next note, but later to avoid the noise the caller used a mercury key). This is also Münsterberg's method. We soon found a very strong tendency to close the key too soon, on the part of the reacter, and too late on the part of the caller; the former presses the key when the voluntary impulse is ready, when he feels that you know what the word is and what it is necessary to do to repeat it, rather than when the vocal mechanism is ready and may act. By this method our times were much too short, centering about 200 σ. The simple reaction time to a sound by closing a key with the finger was for J. J. 148 σ, for F. W. 135 σ. But it is hardly proper to subtract this from the repetion time to obtain the time of the mental process alone. To include the complete mechanical process the stimulus must be a vocal utterance with an accompanying closure of the key with the finger and the same for the reaction. After much trial we conclude to use a small bit of wood held between the teeth and attached to a spring lever, so that the slightest separation of the teeth, (always accompanying utterance) would cause the bit to fly out of the mouth and in so doing to make or break the chronoscope circuit. While the key is not free from objections, it worked very well and we could observe with it no serious tendency to anticipate the true reaction. The simple reaction-times recorded in the table were obtained with this key in the following manner: the observer always uttered the sound "ah" (explosive) and the reacter always used the same sound in reacting, so that the simple reaction includes all the mechanical parts of the process, and whatever error there is in uttering or repeating is contained in all alike. The difference between this and the repetition-time (on the average 68 σ) may thus be regarded as the pure mental repetition process. The further details of method and apparatus offer no peculiarities worthy of record.

A Novel Optical Illusion.

(With the assistance of G. W. Moorehouse.)

If before a rotating disc composed of a large sector of one color and a small sector of another, the two differing considerably in shade (e. g. a dark blue and a light yellow), a rod, held horizontally, be passed up and down, the whole disc seems broken up by horizontal parallel bands of a color similar to that present in greater proportion[1]. This illusion

[1] This illusion was first brought to my notice by Dr. Münsterberg upon my visit to his laboratory at Freiburg. I can find no reference to it in the literature accessible to me.

is especially striking when the component colors are markedly different, with the lighter color forming only a very small portion of the disc, when the disc is in very rapid rotation, the rod very slender and its motion moderately rapid. The bands appear quite as well if the movement of the rod is vertical, oblique, rotary, etc.; the effect of bending the rod into a spiral or other fanciful shape and giving it a rotary movement is especially striking, the bands always following parallel to the outline of the wire. If instead of showing but two colors the disc is composed of three or more the bands appear each composed of several colors; and if a disc composed of small sectors of the seven primary colors be rotated each band presents a rainbow-like appearance. This phenomenon seems especially remarkable when contrasted with the universal tendency of successive optical images to fuse. The mixing of colors upon a disc is itself a typical instance of such fusion. But here there is a sort of separation of colors, and that too at a high rate of rotation. For example, if two rotating discs were presented to us, the one pure white in color, and the other of ideally perfect spectral colors in proper proportion, so as to give a precisely similar white, we could not distinguish between the two; but by simply passing a rod in front of them and observing in the one case but not in the other the parallel rows of colored bands, we could at once pronounce the former to be composite, and the latter simple. In the indefinitely brief moment during which the rod interrupts the vision of the disc, the eye obtains an impression sufficient to analyze to some extent into its elements this rapid mixture of stimuli. The more detailed description and possible explanation of this illusion formed the object of our study as of the present exposition. It will conduce to brevity of description to arrange the several results of experimentation under appropriate headings.

Extent of the Illusion. The illusion appears with any pair of colors, provided only that the two are moderately different; but the resulting bands are of various degrees of distinctness according to the colors used. The result is clearest when the colors are strongly contrasted; we experimented successfully with red, yellow, blue, green, black, white, etc., in various combinations. Of a series of seven shades of green, numbers "one" and "two" were very dark, number "three" considerably lighter than "two," and the rest all very light with only slight differences between them. The bands could not be observed with a combination of "one" and "two" nor with any combination of "four," "five," "six," or "seven," but in all other combinations the contrast was sufficient to cause the illusion. By a differ-

ent method, to be described below, we succeeded in more accurately determining the amount of contrast needed to produce the illusion.

Proportions of the Component Colors. In a disc composed of dark red and light yellow, the bands could just be seen when a sector of 12° of red was combined with 348° of yellow, and remained visible with a decrease of yellow and an increase of red until only 3° of yellow and 357° of red were present. With red predominating the bands are also red but of a red *darker* than the general color of the disc; with yellow predominating the bands are yellow but of a yellow *lighter* than the resulting mixture. The darker bands are always more easily seen and clearer than the light ones, and hence a smaller sector of yellow with red than of red with yellow is needed to produce the illusion. We should infer that there would be a ratio of the two colors at which the bands would be neither darker nor lighter than the background; and in fact there is quite a range of ratios for which the bands are so nearly the color of the background that they are difficult to observe. This range differs for different combinations of colors; for our red and yellow the critical point is about 110° of red and the rest yellow. With more red than this the bands become more and more deeply red, and with less red more yellowish; to this extent the statement that the bands are of the color predominating in the disc must be modified.

Effect of the Width and the Rate of Motion of the Interrupting Rod. The general effect of an increase in the width of the interrupting rod was to render the illusion less distinct and the bands wider; moreover the illusion is more limited in range, i.e., it is confined to a narrower range of rotation rates of the disc and the like. While with a fine wire about a millimeter in diameter, the bands are sharply outlined and striking, with a stick 4 mm. in width they require somewhat of a strain to continuously observe them.

Maintaining the rotation rate of the disc as nearly constant as the clockwork that runs it will allow, we may vary the rate of passing the rod to and fro with characteristically different results. Moving the rod across the disc six inches in diameter, so that each movement from up down, and from down up, corresponded with the beat of the metronome beating 208 times per minute, the bands were about $\frac{5}{8}$ inch apart, with the metronome at 160 per minute about $\frac{1}{2}$ inch apart; with 108 per minute $\frac{1}{4}$ in.; with 80 per minute $\frac{1}{8}$ inch; with 60 per minute less than $\frac{1}{16}$ inch. In other words the bands are separated by smaller and smaller spaces as the rate of movement of the rod becomes slower and slower. The distances

between the bands were estimated by free-hand drawings and then verified by comparison with the rotating discs.

Analysis of the Factors of the Illusion. Allowing the above to suffice as a general explanation of the extent of the illusion, we may proceed to an analysis of its component factors. The factors are (a) the appearance not of one band but of several; (b) the distance between the bands; (c) the color of the bands; (d) the width of the bands; (e) the color of the interrupting rod; (f) the width of the interrupting rod; (g) the rate of movement of the interrupting rod; (h) the rotation-rate of the disc; (i) the nature and proportion of the component colors. It will thus be seen that the illusion is quite complicated. As an important step towards its explanation, we will consider first,

The Time-Relations of the Illusion. This involves the factors (a), (b), (g), (h). Before proceeding further it will be necessary to describe another method of producing the illusion, without which its explanation would have been impossible. This consists in sliding two half discs of the same color over one another leaving an open sector of any desired size up to 180° and rotating this against a background of a markedly different color, in other words we substitute for the disc composed of a large amount of one color, which for brevity we may call the "majority color," and a small amount of another, the "minority color," one in which the second color is in the background and is viewed through an opening in the first. With such an arrangement we find that we get the series of bands both when the wire is passed in front of the disc and when passed in back between disc and background; and further experimentation shows that the time relations of the two are the same[1]. (There is of course no essential difference between the two methods when the wire is passed in front of the disc.) These facts enable us to formulate our first generalization, viz., that for all purposes here relevant the seeing of a wire now against one background and then immediately against another is the same as its now appearing and then disappearing; a rapid succession of changed appearances is equivalent to a rapid alternation of appearance and disappearance. Why this is so we are unable to say, but the fact itself seems well established, and is both

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[1] Of course when the wire is passed between the disc and background the distinctness of the wire depends upon its contrast with the background; it appears of its true color modified by its appearance on the background and by the rotating disc through which it is seen, but it does not assume the contrast effects assumed by the rod moved in front of the disc. The time-relations in the two cases are the same but the color-phenomena considerably different.

novel and interesting. By this "open disc" method we are enabled to study the illusion independently of color, by having the disc of white against a white background with the rod moving between disc and background. In this case, as in the others, we see several rods or bands, and the suggestion is natural that we are dealing with the phenomena of after images; in other words we see the rod through the opening in a certain position, then for a brief time lose sight of it, then see it again in a slightly different position and so on, the after image of the one view not having faded out when the second view is obtained. If this is the true explanation of the fact that several rods are seen, then we should—with different rotation rates of disc and rod—see as many rods as multiplied by the time of one rotation of the disc would yield a constant, i.e., the time of an after image of the kind under consideration. The result of about 20 such tests with varying rates of the disc was the following:

		J.J.	*G.W.M.*
Average time of rotation of disc when	2 images of the rod were seen	.0812 sec.	.0750 sec.
" " " " "	3 " " " "	.0571 "	.0505 "
" " " " "	4 " " " "	.0450 "	.0357 "
" " " " "	5 " " " "	.0350 "	.0293 "
" " " " "	6 " " " "	.0302 "	.0262[1]"

Multiplying the number of rods by the rotation rate we get for J.J. an average time of after image of .1740 sec. (a little over $\frac{1}{6}$ sec.) with an average deviation of .0057 (=3.2%); for G.W.M. 1492 (a little over $\frac{1}{7}$ sec.) with an average deviation of .0036 (=2.6 %). An indepentent test of the time of after-image of J.J. and G.W.M. by observing when a black dot on a rotating white disc just failed to form a ring resulted in showing in every instance a longer time for the former than for the latter.[2]

It has already been observed that the distance between the bands diminishes as the rotation rate and the rate of movement of the rod increases; this suggests that the distance between the parallel bands is that moved over by the rod during one rotation of the disc. This we tested with various rates of disc and rod by spreading a pair a compasses until they seemed to span the distance between the bands. The following is a comparison of the actual and the theoretical result under this hypothesis:

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[1] There is a further point to be considered here, viz.: the size of the aperture, when nothing different is said it was 21°. We repeated some of the above experiments, however, with apertures of 10½° and of 42°, obtaining the same results.

[2] For the method of timing the disc see Note A. The rod was moved between parallel bars to the beats of a metronome.

One revolution in x seconds.	Rod moved y mm. in x sec.	Observed distance between bands in mm.
.0551	18	18
.0220	5.13	5
.0227	5.03	5
.00987	1.48	2
.0233	3.04	3.5
.0250	3.05	4
.0376	5.08	6.5

Considering the difficulties of the observation these agreements are extremely close. Having now accounted for the width of the bands, the distance between them, the fact that several are seen, it remains to examine certain general conditions of the illusion and more particularly the color factors of it.

The Color Factors of the Illusion. A brief acquaintance with the illusion sufficed to convince us that its appearance was due to contrast of some form, though the precise nature of this contrast is the most difficult point of all. It has already been observed that the two component colors must be somewhat different to produce the illusion and that the bands are darker when the majority color is darker than the minority color, and is lighter when the former is lighter. By the following device we succeeded in determining the minimum amount of difference between the colors that would produce the illusion: we used an open disc of light green (aperture 21°) in front of a back ground of the same color and used with the green disc a variable sector of black. When moving a rod in front of this combination we always observe a series of light bands due to the presence of the large amount of green with a little black, but as the black gains a certain proportion, we observe in addition a series of dark bands due to the contrast of the resulting darker green (mixture of light green and black) with the light green of the back ground. We have now only to vary the black till these darker bands may just be seen; this critical point with the colors used proved to be about 24° of black added to 315° of green, or "$\frac{1}{13}$ darker" if we may use that expression. It need hardly be added that the aperture exactly corresponds to the minority color and requires no special consideration.

Colors differ in two senses, in the fact that they are formed by different wave lengths, and that they contain more or less black. We have shown that colors differing only in the latter respect produce the illusion; it remains to be seen whether difference of color alone will produce it. We have the following evidence that it will not: (1) We were able to select a dark red and a dark blue, which did not give the illusion, but in which the substitution of slightly different red or blue, brought it out; (2) the same is true of a light green and light yellow; (3) in many cases while not succeeding in obtaining

colors that would cause the illusion to disappear, we succeeded in finding for any given color a second with which the illusion is faint, and (4) we can effect this more systematically by combining with one of a pair of colors yielding the illusion sufficient white or black to cause it to vanish. In a vague and popular sense we call a given red lighter or darker than a given blue, but the physicist (as we understand it) has no accurate determination of this impressionist estimate; perhaps for ordinary empirical purposes it would be of advantage to call two colors equally dark when they fail to give the illusion now under discussion.

There is a factor in the illusion not yet considered, viz., the color of the interrupting rod. Heretofore this has been a copper wire; and whenever the illusion is distinct the color of the wire is of very slight importance, but when it becomes difficult to observe, then wires of certain colors will produce it and of others will not. The general outcome of much experimentation with colors hardly sufficiently contrasted in shade to produce the illusion is this, that with the component colors both rather dark, whether in proportions giving a light band or a dark one, dark wires will produce it, but light ones will not, with the component colors both light, light wires will produce it but dark ones not.[1] We are unable to bring this result into harmony with the ordinary laws of contrast, and must be content to give the empirical result without explanation.

We have but one further mode of observation that sheds light on the present point. We can obtain the illusion quite as well by substituting for the disc a cylinder covered by a strip of colored paper with a small strip of another color crossing it. We happened to use a rubber band to hold the second strip in place and noticed a deep contrast band parallel to the rubber when in rotation. We substituted a lead-pencil mark for the rubber and still obtained the deep band, *this band being of the same color as the bands* produced by passing a rod before the disc or cylinder. A lead-pencil mark on the disc will have the same effect. We observed however that this appeared only when the line passed across the color present in lesser proportion, which at once suggested (conformably to the experiment with the open disc) that the bands are originated during our vision of the minority color. We tested this by fixing a strip of brass to the disc in such a way that it could be made to rotate on its own centre (by striking against a fixed point) during the rotation of the disc. This device replaced the rod and caused the illusion so long as it

[1] The different colors of wire were simply insulated wires with the colors of the insulation different.

was fixed to the minority color but not when fixed to the majority color. This offers some clue to the kind of contrast involved in the illusion but still leaves room for a satisfactory explanation.

The chief points of our study may be thus resumed:

(1.) The illusion appears whenever the component colors are moderately contrasted in shade, and the one is present in distinctly greater proportion ,than the other; a difference in color, but not in shade, does not produce the illusion.

(2.) For all purposes affecting the illusion (except certain points of color) alternate appearances of an object against different back grounds is equivalent to alternate appearance and disappearance of the object.

(3.) The fact that several bands are seen is due to the persistence of the after image.

(4.) The distance between the bands is the distance through which the rod has passed in one revolution of the disc.

(5.) With the majority color darker than the minority color the bands are darker than the resulting mixture, and lighter when the majorily color is the lighter.

(6.) The width and rate of movement of the rod as well as the rotation-rate of the disc determine the width of the band; the color of the rod becomes important when the illusion is diffcult to obtain, it then appearing that with the dark colors a dark rod is better, with light colors a light rod.

(7.) The bands originate during the vision of the minority color.

(8.) The contrast effect of the bands (while not satisfactorily explained) may also be obtained by a mark upon the minority color.

Accessory Apparatus for Accurate Time-Measurements.

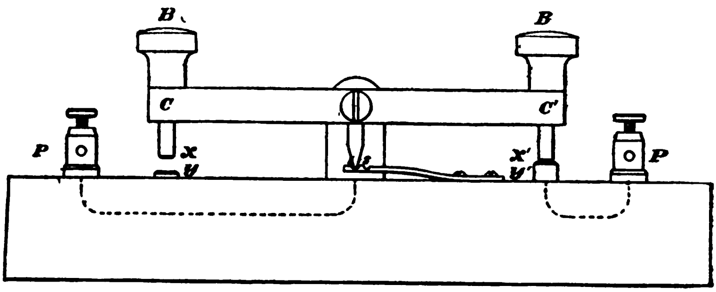
(With the Assistance of Frederick Whitton.)

A large portion of the measurement of the times of mental processes has been accomplished with the aid of the Hipp chronoscope; the objections to the use of this apparatus are the difficulty of its regulation and the possible sacrifice of accuracy to convenience. To secure accuracy, the chronoscope must be constantly controlled, and for this purpose complex devices have been resorted to for ensuring constancy of conditions and the like.[1] To simplify the method of control we

[1] The apparatus supplied with the chronoscope is altogether defective, the mechanical release of the ball is bad, the measurement of the falling height uncertain, the catch by which the circuit is closed imperfect and slow. In addition to these mechanical defects, the range of height is much too meagre. See Wundt's note to the same effect. Phys. Psych., 3d ed., II. p. 276. Note.

succeeded after many trials and failures in perfecting a piece of apparatus by which the error of the chronoscope could be readily and accurately determined. In all methods in which the control consists in timing the fall of a ball (and this too is our method), the general problem is this: two pairs of events are to occur simultaneously, first the ball to begin its fall and the hands of the chronoscope to move, and then at a definitely measurable point of the fall, the hands of the chronoscope are to stop. The difference between the present and all other methods of securing this end (and to which we think its success is due) is that this simultaneity is obtained not by allowing a magnet to release a ball and also a blade held against the action of the spring or the like, but by the use of a special form of key. A simple movement of this key serves to *make* one of two independent circuits and to break the other at the same moment. The explanation of this key will be easier if preceded by a description of the form of key used almost exclusively in this laboratory for finger reactions. It is a key that when once closed remains closed and when opened remains open; this gives the advantage of having the closing and opening movements the same, and allows this movement to be the very natural one of quickly tapping the key and then withdrawing the hand. A brass arm CC' pivoted at its centre upon a brass upright terminates above at each end in a hard rubber button BB', and below in a brass point XX'; projecting from the board upon which the whole is mounted are two brass points YY' for the purpose of making or breaking contact with XX'; finally there is fastened to the arm CC' a wedge-shaped piece of brass playing between the notches 1, 2. The key as pictured is ready to be used to break a circuit, made through the point X' connected with the binding-post P', and the point central support connected through the apparatus with the binding-post P. A simple pressure of the finger at B' breaks the contact at X' Y', and forces the wedge into the position (2), in which it is securely held by the notch (2). When in this position it is ready to be used to *make* a circuit by a pressure upon B'; it can only assume one or the other of these two positions, and in either case is securely held in place. Now imagine that the button B' instead of being of hard rubber is of brass, and imagine the end of a second brass lever at right angles to CC' in position to press down upon B' and thus establish a circuit between B' and the second lever; imagine further that the blow of this second lever upon B' is given by the release of a strong spring that holds everything firmly until X comes in contact with Y, and the apparatus is complete. A release of the spring thus establishes one circuit through B' and the second lever which sets

the chronoscope going, and at the same moment the ball begins to fall by the breaking of the circuit at X' Y'. The circuits are entirely independent and supplied by different batteries. To test the simultaneity we make our connections so that the making of the one circuit sets the chronoscope going and the breaking of the other stops it; and in no case did the chronoscope hands show the slightest tendency to move.



The apparatus controlling the fall of the ball is simple. An electro-magnet tapering to a point at one end is tightly held in a bracket, adjustable along a vertical slide, which in turn is securely fastened to the window frame. It is important that all parts of this be strong and securely fixed to the wall of the building. The slide is 6 to 7 feet high so that a fall of .6 to .7 seconds can be measured. From the value of g at Madison we calculate from the formula $s=\frac{1}{2}\ g\ t^{2}$ the heights at which the ball should just consume .1, .2, .3, .4, .5 and .6 seconds in its fall and mark these points on the millimeter scale along the slide, making our readings by aid of a fine wire. The ball of soft iron about $\frac{3}{4}$ of an inch in diameter is held at the tip of the magnet and in its fall strikes against the arm of a well-balanced lever, and thus severs an electrical connection by which the clock comes to a standstill; while the distance between the upper surface of the lever and the lower surface of the ball is the space fallen through in the measured time. Two further points may be noted; first to find the zero point on the scale let the magnet hold the ball and move the bracket down until the ball just touches the lever sufficiently to break the connection, and mark the point opposite the wire zero; second, use three or four thicknesses of tissue-paper between the ball and the magnet to separate the surfaces and thus diminish the time of demagnetization. With this apparatus one can without assistance take half a dozen records at different points in as many minutes; and in the work described above ten observations were recorded before and after each day's work, from which the

error of the chronoscope for the day was calculated. As the observations were taken from all six positions—.1 to .6 sec. (in the latter portion of the work for four positions) we could determine whether the error was constant or relative and found the former to be the case. Throughout a period of six months, during which the chronoscope was tested, its maximum error was .005 seconds and the average error about .002 seconds. The position of the springs regulating the chronoscope was always noted and by changing these the error could be reduced to practically zero. But we aimed not at absolute accuracy but at an accurate determination of our daily error. This apparatus has proved itself so easy of manipulation and so time-saving, that its use is confidently recommended to experimental psychologists.

Note A.—On the Timing of Rotating Discs. A simple and fairly accurate means of determining the rate of rotating discs, especially of those rotating by clockwork, has long been a desideratum. The ordinary speed-counter is out of the question on account of the great friction involved. The "interruption-counter" invented by Dr. Ewald of Strassburg is a device by which each making of an electrical circuit moves the hand of a dial just one division, the dial showing 100 divisions; its original purpose was to count the vibrations of a tuning fork and thus to serve as a convenient form of chronoscope. It is capable of counting the vibrations of a fork with a vibration-rate of 100 per second, but for this, great delicacy of manipulation is necessary. Its adaptation to the present purpose is simple, though quite a number of devices were attempted before the simple one was obtained. Two small platinized tips were soldered at opposite points on to the circumference of the wheel of the clockwork next to the one to the axle of which the disc is attached. A light brass blade, also platinized, is suspended from above with a thumb-screw regulation, so that the tips on the wheel just make a coutact with it as they pass it. As this second wheel revolves once to every eight rotations of the disc we can count to the nearest four rotations, which is quite accurate enough. By increasing the tips we can count every two or every rotation, though the adjustment must then be finer. We allow the current to run through the counter for 15 seconds (as counted by the second hand of a watch) by closing and releasing a mercury key. We also devised a method by which the timing was done automatically and so one person could observe the disc and take the time measurements as well. This consisted in fastening to the ends of an ordinary revolving drum a circular piece of paper with a strip extending over about 180° cut out; by placing the end of a fine wire

opposite this paper it is easy to arrange one's circuits so that during the time the wire touches the brass of the drum the counter is recorded, while for the rest of the revolution of the drum the current is intercepted by the paper; finally we set the drum so that the time of contact is a convenient one, say 15 sec., and when we see the contact approaching close, we lock the key and go on with our observation. The counter then of itself begins to record, does so for exactly fifteen seconds and stops; and we can make the reading at our leisure. For all these purposes the counter proved itself an exceedingly valuable apparatus.

As this is one of the first of these instruments to be used, our experience with it may be of advantage to others. Its two defects are that the wire on the magnets is too fine, thus causing an excessive resistance, and that the spring by which the magnet blade is withdrawn is not adjustable. After remedying these defects we were able to successfully manipulate the instrument with a single storage cell battery and very little trouble. We also tested the apparatus with a tuning fork of 100 per second and found it reliable. If the instrument were made as large again its efficiency would be increased.

Note on a device for color mixing. One objection to the ordinary method of mixing colors by forming sectors of them upon a disc and rapidly rotating it, is that while the mixture is produced one cannot readily compare the result with the original component colors. It is as a corrective of this defect that the following device is suggested. It consists simply of using a half disc (or any other desired portion most easily obtained by sliding two half discs or four quarter discs upon one another) of the one color and during its revolution holding the other color in back of it to one side. Then on either side you have the original colors, and where the two overlap the resulting color; if the colors be red and blue, you have before you on either side the red and blue and between them a purple. One can hold two (and with proper arrangements more) different colors in back of the same rotating disc and thus show for instance the mixture of blue with red and blue with yellow and the original blue, red and yellow all clearly displayed in line. One can show the mixture of an entire series of colors with the same color without stopping the disc, and for matching a given color with a resulting mixture this is especially convenient. With two rotating discs, overlapping upon a common background one can show the result of mixing three colors and the three original colors at the same time, but there the manipulation is no longer so simple. The method is easily adapted to the

fusion of other visual impressions and is particularly suited to class-room demonstration. A clockwork for rotating the disc is a great convenience in the experiment.

THE PSYCHOPHYSIC SERIES AND THE TIME-SENSE.

(With the assistance of W. B. CAIRNS.)

In an earlier paper reporting the studies made in this laboratory (AMER. JOURNAL OF PSYCH., Vol III, No. 1, pp. 44-49) it was shown that when we attempt to sort out sizes of sticks into six or nine magnitudes, either by the eye or by passing the finger over the sticks, the result is that the average lengths of the sticks of the several magnitudes are separated by approximately equal differences; i. e., they form an arithmetical series. This method was spoken of as that "of the psychophysic series," and consists simply in distributing according to a general impression a large variety of sense-impressions into classes or magnitudes; it is also the method by which the stars were divided into their magnitudes. If the psychophysic law holds when thus tested the result would be, as it notably is, in case of the stars[1] that the ratios of the averages of neighboring magnitudes would be constant, i. e., would form a geometrical series. A suggestion of an explanation of the applicability of the law to star magnitudes and its failure in magnitudes of extension both visual and tactual-motor, was recorded in the former study in the following words: The law may be expected to apply to "such sensations as are appreciated *en masse*, and with not too distinct a consciousness of their intensity [or extension]; when the sensation is a sort of impressionist reception of the gross sensation without dividing it up into units, or conceiving it as so composed, we may expect the law to hold good. This would be the case with the rough estimations of star brightnesses." To further test this point of view we experimented with the perception of time-intervals, in which as in the estimation of star magnitudes there is an unanalyzed appreciation of the interval, without regarding it as composed of constituent units; and for which, according to the above suggestion, the law should hold good.

Accordingly we set a metronome at one of many intervals and asked the observer after he had listened to its beating as long as he desired in order to determine his judgment, in which of *six* classes of intervals he would place it. At the outset the observer was allowed to listen to the slowest interval, 40 per minute, and to the fastest, 208 per minute, and to

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[1] See the proof of this in an article Vol. I, No. I, p. 112 of this JOURNAL.

imagine this range divided up into six grades or magnitudes. At first the assignments are somewhat vague and variable but they soon became relatively fixed, though there is considerable overlapping of the various magnitudes even in the best observers. Much of this is undoubledly due to contrast, an interval following a very long one seeming shorter than it would if following a short one, and the like. We used intervals rising by 2 per minute from 40 to 120 per minute, by 3 per minute from 120 to 144, and by 4 per minute from 144 to 208, thus using in all 63 intervals. These were written on small square cards and three sets of such, or 189 cards, were used at one sitting with each observer, the cards being tossed in a box and drawn at random, and the metronome set according to the number drawn. The longest time intervals, i.e., from 1.5 seconds down were called magnitude I, and the shortest from .29 seconds up, magnitude VI; the observer sat with his back to the metronome, knew nothing of the experiment except what were the longest and the shortest intervals, and simply called out the number of the class to which he assigned the given interval. Three such full sets of nearly 200 observations were made on one observer, two each upon two others, and one each on three others, making ten in all. When the results are obtained we collect all the intervals assigned to each of the magnitudes and find the average duration of the magnitudes of that interval, which averages will serve as the basis for the present discussion.

In the accompanying table are shown for each set of observations the average number of beats per minute of each magnitude, with the number of observations contributing to that average following it in small figures the successive differences and ratios of these magnitudes, and the average of these differences and ratios together with the average deviation from them expressed in percentages. At the foot of the table a similar series of weighted averages (i. e., results of multiplying each average by the number of observations and dividing by the total number of observations), is given, combining all the observations, and this we shall first consider. To test whether the series approaches an arithmetical or a geometrical series, we have simply to compare the *constancy of the differences with that of the ratios*. This may be done with sufficient accuracy for the present purpose by finding for each of the five results the differences from their average, dividing this by five, and expressing it as a percentage of the average of the five differences, or the five ratios. We thus see that while *the average variation from a constant difference is 24.8 per cent., the average variation from a constant ratio is only 4.2 per cent., indicating a decided approximation to a*

Magnitudes.		I.	II.	III.	IV.	V.	VI.	Aver-age.	Average Deviation	Ratio.
I.	W.B.C.	50.1^{29}	73.0^{29}	94.6^{30}	112.5^{34}	142.2^{28}	181.8^{37}			
	Differences, Ratios,	22.9 1.437	21.6 1.299	17.9 1.190	29.7 1.264	49.6 1.330		28.3 1.304	31.9% 4.9%	1 : 6.51
II.	W.B.C.	44.5^{23}	67.5^{29}	95.1^{44}	119.0^{35}	151.0^{24}	185.2^{32}			
	Differences, Ratios,	23.0 1.501	27.6 1.423	23.9 1.251	32.0 1.274	34.1 1.226		28.1 1.335	13.0% 7.6%	1 : 1.71
III.	W.B.C.	48.1^{21}	67.9^{29}	96.0^{47}	115.9^{43}	160.8^{28}	190.8^{22}			
	Differences, Ratios,	19.8 1.412	28.1 1.414	19.9 1.207	44.9 1.388	30.0 1.187		28.5 1.322	24.0% 7.5%	1 : 3.20
IV.	J. J.	48.0^{21}	64.1^{26}	89.2^{33}	106.4^{35}	140.5^{39}	186.1^{33}			
	Differences, Ratios,	16.1 1.335	25.2 1.392	17.2 1.193	34.1 1.320	45.6 1.325		27.6 1.313	35.4% 3.7%	1 : 9.57
V.	J. J.	50.6^{35}	77.7^{31}	97.6^{30}	118.5^{30}	146.0^{26}	184.1^{36}			
	Differences, Ratios,	27.1 1.535	19.9 1.256	20.9 1.214	27.5 1.232	38.1 1.261		26.7 1.300	18 8% 7.3%	1 : 2.57
VI.	R.H.T.	44.2^{9}	57.9^{29}	74.0^{37}	92.2^{44}	131.4^{43}	176.3^{44}			
	Differences, Ratios,	13.7 1.310	16.1 1.278	18.2 1.246	39.2 1.425	44.9 1.342		26.4 1.320	48.1% 3.8%	1 : 12.66
VII.	R.H.T.	44.4^{10}	54.0^{17}	81.8^{44}	105.0^{44}	132.7^{33}	178.9^{39}			
	Differences, Ratios,	9.6 1.515	27.8 1.216	23.2 1.284	27.7 1.264	46.2 1.349		26.9 1.326	31.2% 6.6%	1 : 4.73
VIII.	F. St.W.	48.1^{30}	69.4^{39}	98.0^{34}	123.6^{37}	157.6^{36}	183.1^{23}			
	Differences, Ratios,	21.3 1.443	29.6 1.427	25.6 1.261	33.4 1.270	26.1 1.166		27.2 1.313	12.6% 7.4%	1 : 1.70
IX.	S.S.B.	49.1^{24}	69.0^{31}	95.3^{46}	122.6^{35}	158.7^{28}	193.0^{39}			
	Differences, Ratios,	19.9 1.405	26.3 1.381	27.3 1.286	36.1 1.294	34.3 1.216		28.8 1.316	17.8% 4.6%	1 : 3.87
X.	S.D.J.	49.8^{29}	77.1^{33}	98.2^{35}	120.3^{40}	153.7^{28}	188.9^{29}			
	Differences, Ratios,	27.3 1.548	21.1 1.274	22.1 1.225	33.4 1.277	35.2 1.223		27.8 1.308	18.6% 7.2%	1 : 2.58
Weighted Average.	Weighted Average.	$50.0^{28\ 1}$	$69.6^{28\ 6}$	$93.6^{37\ 0}$	$114.3^{37\ 6}$	$147.7^{30\ 6}$	$185.1^{31\ 8}$			
	Differences, Ratios,	19 6 1.392	24.0 1.345	20.7 1.221	33.4 1.292	37.4 1.253		27.0 1.301	24.8% 4.2%	1 : 5.90

geometrical series, and therefore, according to expectation, an obedience to the psychophysic law. In the last column of all, the ratios of each pair of average deviations are given, and for the general result (accepting this rough mode of comparison), we have this, namely, that the approximation is six times (5.90) as close to a geometric as to an arithmetic series.

We may instructively note too a few peculiarities of these results; first, that while the ratios of neighboring magnitudes are approximately constant, there is a tendency for these ratios to decrease slightly from I to VI, or to increase in passing from short intervals to long ones. A precisely similar result is found in the case of star-magnitudes; and in the latter case the observations are sufficiently extended and regular to warrant an emperical formula expressing the rate of increase of this ratio, with an increase in brightness of the star-magnitudes. Moreover, two further irregularities recorded in the study of star-magnitudes reappear in the present study. The first is that at one extreme the ratio tends to be unusually large, and at the other unusually small. This is due to the limitations of the series, and the fact that were there another magnitude at each end of the series, some intervals now placed in I or VI would be placed in the class below I, or in that above VI. The errors thus induced are evidently opposite in direction. The tendency is more marked in the star observations than here, but if we note the individual results we see that in seven of ten cases the ratio of I to II is markedly larger than the others, and in five cases the ratio of V to VI is appreciably smaller than the others. These peculiarities are good evidence of the similarity of the psychological processes employed in sorting stars and in classifying time-intervals with magnitudes. A marked peculiarity of the present series (and one that interferes seriously with its regularity), is the tendency to make only a slight division between intervals assigned to III and those assigned to IV, but a marked one between those assigned to IV and those to V. This tendency is present in nine of the ten sets, and is marked in six, and so can hardly be accidental. It seems to depend upon a habit of viewing III and IV as medium intervals, while V is already a short interval. A closely similar irregularity was found in the estimations of the star-magnitudes of Ptolemy and Sufi.

Regarding the individual results we notice considerable irregularity, some individuals maintaining the law much more closely than others, as is observed most readily by a view of the last column of the table. That much of this irregularity is due to the paucity of observations is indicated by the fact that the average deviations in the combined sets I, II, III, of

W. B. C., IV, V, of J. J. and V, VI, of R. H. T., are smaller than the average of the group of three or of two sets. Thus for W. B. C.'s three sets the average variation from a constant ratio is only 4.8 per cent., in J. J.'s two sets 4.2 per cent., in R. H. T.'s two sets 3.3 per cent., while the ratios of the average variations from a constant difference and a constant ratio becomes as 1:5.54, 1:6.62 and 1:10.97. It should also be noted that the number of intervals assigned to each magnitude differs considerably. In the general average the deviation from the average of 31.3 for each magnitude is 13.4 per cent. III and IV have most intervals assigned to them (perhaps because many doubtful ones are naturally assigned to the medium magnitudes). I and II have fewest.

One further point may be mentioned as supporting the supposition that with a more conscious analysis of time-intervals, with the establishment of a habit of estimating time by seconds, the tendency to follow the geometrical series will be diminished. Thus it is quite noticeable that the first sets of all three observers who went through more than one set approach more closely to the psychophysic series than the later ones, the average deviations in the two cases being about as 4 to 7. Perhaps this is accidental, but it certainly suggests a departure from the impressionist method of estimating intervals with which we set out. Of the remaining three records VIII is unsatisfactory and was so noted at the time, while IX and X are records of observers accustomed to astronomical work, in which the second and half-second interval is important. The acquired habit of analyzing time intervals according to standard units may thus account for their slight tendency to follow the psychophysic series in their case.

The result of the present study thus goes to support the suggestion that when we estimate sensations roughly and on general impressions, without comparing them with standard units, we naturally, though unconsciously, make use of a geometrical series. We make relative distinctions rather than absolute ones, and this is the natural basis of the psychophysic law. While the process is a rough one, and is often accompanied by much hesitation and little confidence, the average results are fairly clear, and add one more to the many illustrations of the statistical regularity of apparently lawless and entirely unconscious mental tendencies.

THE PSYCHOPHYSIC SERIES AND THE MOTOR SENSE.

(With the assistance of AUGUSTA A. LEE (MRS. CHARLES GIDDINGS).

As a further application of the method of the psychophysic series we experimented with a form of movement in which with the forearm supported at the elbow as a pivot the hand

moved laterally for practically any distance from 5 to 190 millimeters. The extent of the movement was limited and measured in the following way: The hand held a glass pencil and supported the same along a straight edge, the pencil furthermore moving in the ridges of very finely grooved glass. Over this glass was mounted a skeleton triangle about 6 inches across the base and 20 inches in altitude, and the whole moved in a slide to or away from the hand holding the pencil, such movement limiting the pencil to movements of varying length between the sides of the triangle and parallel to its base. A scale at the side indicated for each position of the triangle the distance moved over by the pencil. After allowing the subject to move over the shortest and the longest distances he was asked to mentally divide this range into six classes or magnitudes, and assign the various distances presented according to the perceptions gained through the sense of motion (sight was of course excluded), to the various magnitudes. Though the average lengths of these magnitudes present considerable irregularity, they very clearly show that they do not accord with the psycho-physic law and that they roughly approximate an arithmetical series. The averages themselves, together with the number of observations contributing to the average, are given in the following table:

	I.	II.	III.	IV.	V.	VI.
A. A. L. (1)	14.8 (47)	40.5 (45)	75.6 (28)	100.6 (18)	135.8 (21)	166.8 (20)
A. A. L. (2)	20.9 (42)	58.5 (34)	93.7 (26)	123.4 (15)	152.0 (12)	169.5 (13)
E.	13.5 (16)	36.6 (20)	70.7 (25)	110.7 (16)	134.8 (17)	168.6 (5)
H.	15.7 (27)	48.3 (28)	80.3 (28)	121.4 (18)	156.5 (13)	181.0 (8)
J. (1)	9.6 (22)	30.5 (44)	60.6 (45)	89.1 (33)	120.4 (24)	144.8 (39)
J. (2)	7.8 (13)	25.0 (25)	53.6 (30)	84.6 (25)	112.5 (13)	150.9 (34)

To show how far these results favor an arithmetical and how far a geometrical series it will perhaps be sufficient to state the average deviation from a constant difference and from a constant ratio of each of these series, expressed as a percentage of the average difference and the average ratio of neighboring pairs of results.

	L. (1)	L. (2)	E.	H.	J.(1)	J.(2)
Average deviation from a constant ratio—	29.8	30.7	31.1	32.5	34.9	33.7
" " " " " difference—	1.33	17.9	19.3	19.0	12.3	16.6

This shows about twice as close an approximation to an arithmetical as to a geometrical series. If however, we take the average of all six series of each magnitude we obtain a much more pronounced obedience to an arithmetical series; the successive differences become 26.2, 32.5, 32.6, 30.2, 28.3, and the average deviation of these from a constant ratio is but 8.6 per cent. of their average value. Finding the average deviation from a constant in all six series we find no such reduction. It is 30.9 per cent.

If we take into account the varying number of observations contributing to each average by weighting each difference with half the sum of the number of observations of the two averages, the difference of which is expressed, we obtain a still closer approximation to an arithmetical series. For the various series the average deviations from a constant difference then become in percentages:

L (1)	L (2)	E.	H.	J. (1)	J. (2)
10.8	18.1	19.0	13.1	12.3	17.4

and for the combined average of all only 6.3 per cent. It may be noted too that the combination of L's as of J's two sets of observations conform more closely to the arithmetical series than either one, and that the greatest deviations from the constant ratio are apt to occur in the two extremes of the series, when the shortest and when the longest lengths are concerned, the reason of which is obvious. Incomplete as these results are, they are perhaps sufficiently definite to suggest strongly the inapplicability of the physo-physic law (when thus tested), to spatial impressions gained by fore-arm movements. These movements are not altogether dissimilar to those made in running a finger along a stick (v. these studies in this JOURNAL, Vol. III, p. 47), and in both cases the judgment of length is rather conscious and referred more or less definitely to units, probably to notions gained through knowledge of feet and inches. They thus form an additional corroboration of the generalization that we conform to the requirements of the psycho-physic law in gross, *en-masse*, analyzed impressionist judgments, but not in precise detailed, analyzed and considerate judgments. The experiences of a civilized environment have transferred many forms of sense-judgment from the former to the latter class, among them spatial judgments both visual and motor. In these, absolute differences become of equal, and, at times, greater importance to us than relative ones.

THE INTERFERENCE OF MENTAL PROCESSES — A PRELIMINARY SURVEY.

(With the Assistance of W. B. CAIRNS.)

The general field with which the present study deals, (though in a somewhat eclectic and tentative manner), is the power of carrying on two mental processes at the same time. How far, we naturally ask, is this possible, how far economical? How shall we conceive this mental simultaneity, how cultivate and develop the power? We know that the shortening of mental processes brought about by practice is largely due to the power of doing two things at once, is an

overlapping of mental processes; we know, too, that when processes become automatic they may accompany more deliberate and reasoned processes without interference; and we further recognize that certain processes directed to a common end are almost as easily performed together as separately. On the other hand we observe that states of extreme concentration are characterized by immobility, even by a slackening of the automatic functions; we observe the various kinds of disturbance all indicating the interference of two or more mental processes; and we appreciate the necessity of dividing our work into small parts so that they may be easily absorbed and not over-tax our powers. In entering upon this general problem, we at once encounter the difficulty of defining the mental unit; what is a mental process? In a certain sense we are always doing two things at once; the rhythmical functions of circulation and respiration go on while we work; we walk and think, we eat and talk, we write and listen at the same time. In every game of skill several senses act at once; the eye and hand, the ear and mouth, taste and smell act together and aid one another. On the other hand, however, in an intense attention to some fascinating event we stand motionless and almost stop breathing; many persons when thoroughly interested while talking upon the street involuntarily slacken their pace, or stop altogether; few of those who illustrate their remarks by off-hand sketches can talk and draw at the same time, and so on. Our general inquiry is "What processes hinder, what aid one another;" the present study makes no attempt to answer this most important query, but simply describes a few facts and suggestions relating to a very small and special portion of the general field.

We choose as the two types of process, (1) the performance of finger movements, involving rhythm and counting, and (2) of such processes as adding and reading under various conditions. The former were written (by the usual method of a system of Marcy tambours) upon a rotating cylinder, while for the latter we simply noted the time of a set task, performed as rapidly as possible. Our records are in no case very full, and the conclusions drawn are suggestive rather than final. We will consider first the effect upon the movement of an accompanying mental task.

The chief movements used were:

(1) A regular beating with the finger at any rate the subject chose; this we speak of as an *ad libitum* movement.

(2) A movement as rapid as possible and still regular; this is a maximum movement.

(3) Beating in groups of 2s, 3s, 4s, 5s or more.

(4) Beating in alternate groups of 3s and 2s, and 6s, 4s and 2s.

(5) Keeping time to a metronome at different rates, to an air hummed to oneself, etc.

The method by which the effect of mental tasks upon these movements was estimated was to compare the *ease*, the *regularity* and the *time* of these movements when accompanied and when unaccompanied by mental operations. Our results are not sufficiently numerous to show carefully all those effects (time, ease and regularity), but in general certain tendencies are evident. The ease is shown not alone by the feeling of difficulty, but as well by the presence of errors, varying in kind and degree; so, too, even when the rhythm is maintained, it may be more or less irregular, and in turn this irregularity manifests itself in a slowing of the movements. This slowing up is the natural accompaniment of difficult processes. It will thus be seen that these three indications are closely connected with one another, each being in a measure indicative of the others and all evidencing the same points. The "normals" or times of movements with no accompanying mental process are naturally variable. The records upon six days for J. J. of an *ad libitum* movement were 335 σ, 320 σ, 318 σ, 518 σ, 388 σ, 424 σ, 326 σ, while, when several records were taken in the same day, the variations were much slighter in extent. The rate of maximum movements is much more constant, as the following records (of J. S.) show: 152, 163, 140, 148, 160, 164 σ. For beating in groups of 5 the records (of J. J.) have the following times: 1837, 1966, 1801, 1734, 1471 σ, and so on. These figures may perhaps suffice to illustrate the range of constancy of the phenomena in question.

Our first query will be: How far (neglecting for the moment the nature of the accompanying mental operation) will various movements be interfered with by the accompanying process? Our facts suggest the conclusion that the simpler movements are less interfered with than the more complex ones; the records of *ad libitum* movements show no appreciable difference when accompanied or when unaccompanied by other tasks; maximum movements are always somewhat slackened by the accompanying task; beating in groups of 2s, 3s, 4s or 5s become successively more and more interfered with by accompanying mental processes, such interference appearing not very much in a modification of time, but in the irregularity, the presence of errors (there being as a rule more beats in a group then there should be) and in the feeling of strain; in such movements as beating in groups of eleven, of alternate 3s and 2s or 6s, 4s and 2s, frequent failures set in, and when the result is fairly successful, the time is increased and the record more or less irregular. We are unable to range the

various movements in their order of relative difficulty by the amount of interference, but the extremes are very markedly differentiated.

Our second query relates to the amount of interference of different mental tasks. Reading words in construction, reading words disconnected, reading numbers and adding numbers were the chief types of processes used; of these, reading words in sentences is by far the easiest task, all the others tending to make the subject have each beat coincide with a word or addition, and thus slowing the process. Furthermore, any of the movements involving counting, (particularly alternating 3s and 2s and the like) were more interfered with by adding than by reading. But the most striking difference depends upon the manner of going through the mental process, that is, whether the reading, etc., is done aloud or to oneself. In the former case the interference sets in much sooner and is much more serious than in the latter. Even quite simple movements are rendered irregular by reading or adding aloud; and such movements as beating in 3s and 2s or 6s, 4s and 2s were practically failures in such a case, though very successfully done with silent reading. An intermediate process of mumbling seemed to yield an intermediate degree of difficulty. The interference manifests itself clearly in an increased effort, a great irregularity and presence of errors, and a lengthening of the time of movement. Motor processes thus seem to interfere with motor ones, while refraining from movement during intellectual effort would be helpful. Passing now to the effect of an accompanying movement upon the time of such operations as reading sentences, words or numbers, adding (both aloud and to oneself); our data are meagre, but the following suggested inferences, together with the facts that suggest them, may be noted.

(1) The time needed to perform these mental processes is distinctly increased by such accompanying movements, the extent of the increase depending upon the complexity of the movement. (The general average of all the records (107) shows an increase of 4.28 seconds or 30.8 per cent.; J. J., 6.5 seconds or 26.5 per cent.; W. B. C., 6.02 seconds or 36.6 per cent.)

(2) Comparing the process of adding with that of reading, the former is the more complex, and seems to be more interfered with by the accompanying movements. (Comparable records are only about a half-dozen of J. J.'s in which the percentages of increase are about as 40 per cent. to 30 per cent.

(3) Reading and adding aloud are slightly more interfered with by the movements than the same processes performed to oneself. (In six dozen records of J. J., the percentages of in-

crease in the two cases are 31 per cent. and 24 per cent.; in W. B. C., the result is obscured by other factors.)

(4) Of the effect of different kinds of accompanying movements the following may be mentioned.

(a) If the movements are rhythmical beats arranged in groups, like a line of verse or a measure of music, the time increases with the number of beats in a group. For W. B. C., with groups of 2, 3, 4, 5, 6, the times of reading the same passages were 10.4, 11.0, 13.8, 14.0, 15.4 seconds. In one case groups of eleven were attempted with an increase above the normal of about 80 per cent. A similar result appears, too, in attempting to keep time to a beating metronome every 2d, 3d, 4th or 6th stroke of which is marked by a bell, with the accented syllable to coincide with the stroke of the bell.

(b) Simple regular beating, whether to the accompaniment of a metronome or without, can be done without increase of time for reading or adding; for J. J. this is true independently of the rate of the interval. Indeed there is some evidence that a maximum rate of beating also hurries up the mental process. The movements that retarded the processes most were beating in groups of eleven, making three beats of the right hand correspond to one of the left, and beating in groups formed by a six, a four and a two in turn.

(5) Reading disconnected words is more interfered with than reading words forming sense; part of which is due to the tendency of making each word correspond to the beat. While all these points require further corroboration, our results are sufficiently suggestive to evidence the promise of research in this direction. The next step would be to make a detailed study of a few types of interference and accumulate sufficient records to allow of quantitative expression. This it is hoped will be undertaken upon some future occasion.

THE SIZE OF SEVERAL CRANIAL NERVES IN MAN AS INDICATED BY THE AREAS OF THEIR CROSS-SECTIONS.

HENRY H. DONALDSON AND T. L. BOLTON.

(From the Neurological Laboratory of Clark University.)

On several of the cranial nerves of man we have measured the areas of cross-sections, taken at definite points, and sought by this means to get a numerical expression for the size of these nerves. The immediate reason for the investigation was the desire to compare with normal material the cranial nerves of the blind deaf-mute Laura Bridgman in order to determine in her case how far these nerves departed from the normal size. The relation of the size of the cranial nerves to the other structures with which they are associated is a matter of much interest, but one to which, at the moment, we have nothing to contribute.

Little importance seems to have been attached to the size of these nerves by those authors whom we have been able to consult. In general the text books have nothing to say on the subject. Schwalbe ([1]), v. Gudden ([2]), Salzer ([3]) and W. Krause ([4]) have measured the area of the cross-section of the optic nerve in man, for the most part near the bulb, and have obtained areas as small as 7.09 sq. mm. Obersteiner([5]) gives the average area as about 9 sq. mm. Since, however, our sections and theirs were not made at similar points on the nerve, a detailed comparison is unnecessary. In addition to the Bridgman specimen the material employed consisted of seven male and three female encephala. A few brief statements will be necessary by way of comment upon the Table I. in which we embody our results.

Only the first, second, third and fourth nerves have been studied. The olfactory bulb was sectioned where it was thickest. The olfactory tract where it was thinnest. The optic nerves about 10 mm. from the chiasma. The oculomotor nerves about 10 mm. from their superficial origin and the trochleares at the point where they lie on the lateral aspect of the brain stem.

In forming the table the distinction between the nerves of the right and those of the left side is neglected, but the

SERIES THIRD.

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FROM THE AMERICAN JOURNAL OF PSYCHOLOGY.

Vol. 4, No. 4. April. 1892.

STUDIES FROM THE LABORATORY OF EXPERIMENTAL PSYCHOLOGY OF THE UNIVERSITY OF WISCONSIN.—II.

By Joseph Jastrow, Ph. D.

A Study of Zöllner's Figures and Other Related Illusions.

(With the assistance of Helen West.)

The present paper describes an investigation of an illusion which, while familiar and frequently studied, remains in its essence and conditions of origin quite unexplained. We make no claim of furnishing an adequate and final explanation, but simply aim to establish a few steps in that direction. The illusion is that so well marked in figure 1, first described by Zöllner[1]. In this figure the main lines appear very far from parallel; each adjoining pair of lines seems to converge at one end and diverge at the other. Here we have a com-

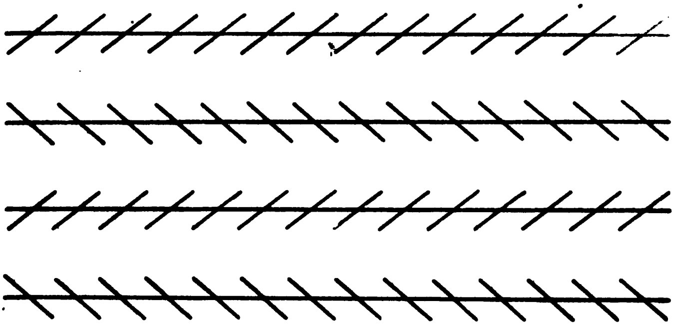


Fig. 1.

plex form of the illusion involved, and it was our problem to ascertain the preceding members of the series of

[1] We are indebted for the use of figures 10, 11, 12, 13 and 24 to the courtesy of Messrs. Charles Scribner's Sons, publishers of Ladd, Outlines of Physiological Psychology, and for figures 1, 14, 15, 17, 18 and 25 to Messrs. Henry Holt & Co., publishers of James's Psychology, which courtesies we acknowledge with gratitude.

which this is the end term. It would be tedious to describe the various steps by which we stripped this figure of one and another of its complications, determining in a variety of ways what part they played in the total effect; it will be more acceptable to substitute for this rather laborious process an exposition beginning with the simplest type of the underlying illusion, and building it up step by step to its most complicated form.

When viewing two lines separated by a space, we are able to connect the two mentally and determine whether they are or are not continuations of one another; but if we add to one of the lines another meeting it so as to form an angle, the lines which seemed continuous no longer appear so, and those

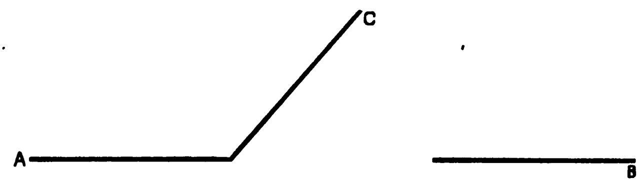
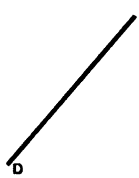


Fig. 2.



which were not continuous may appear so. In Fig. 2 the continuation of the line A appears to fall below the line B, and similarly the continuation of C apparently falls to the right of D. But in reality A is continuous with B, and C with D. If we cover the line C, A and B seem continuous; thus indicating that the illusion is due to the angle. What is true of obtuse angles is true, though to a less extent, of right angles and of acute angles; in brief, the degree of this illusion of discontinuity increases and decreases as the angle increases and decreases. The figures to prove this the reader can easily supply; further illustration thereof will appear later. This is the simplest form of a sense deception that underlies very many familiar but more elaborate figures. The principle therein involved we generalize as follows: Calling the direction of an angle, the direction of the line that bisects it and is pointed toward the apex, then *the direction of the sides of an angle will be deviated toward the direction of the angle.* A very important corollary of this main generalization emphasizes the point, that just as the deviation of direction is greater with obtuse than with acute angles, so also when

obtuse and acute angles are so placed as to lead to opposite kinds of deviation, the former will out-weigh the latter, and the illusion will appear according to the direction of the obtuse angle.

We proceed to notice a few of the means by which the illusion may be varied and tested. A relatively large distance between the lines, the continuity of which is to be judged, produces a more marked illusion than a relatively small distance. The appropriate figures the reader can readily supply. In other words, opportunity must be given for the eye to lose itself in passing from the one line to the other. The degree of illusion may be increased by increasing the number of angles in various ways. We may draw a series of oblique lines parallel to the line *C* (in Fig. 2) and joining the

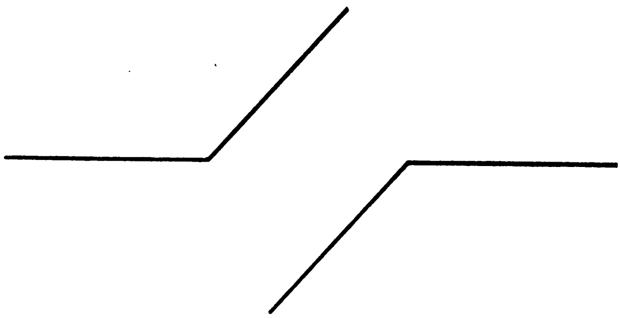


Fig. 3.

line *A*. Or again we may draw a line parallel to *C* from the left-hand end of line *B*. This gives Fig. 3, in which the two horizontal lines seem to be on entirely different planes. The direction of the deviations induced by these angles being opposite in tendency, the result is quite marked. Again we may add a second line parallel to the real continuation, which will be the apparent continuation, and we may further strengthen the tendency to regard the non-continuous line as the true continuation by shading them alike or otherwise differentiating them. Again, we may draw this second line slightly

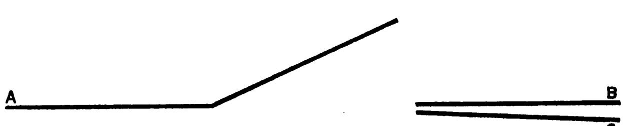


Fig. 4.

oblique instead of parallel with the first line with good success; this is done in Fig. 4. Both of these tests can be made

accurate by measuring the maximum deviation between the parallels or between the parallel and the adjacent oblique line, which the eye will tolerate and still retain the illusion of the false continuation; or again the angle alone might be drawn and the error measured, which the subject would make in adding what appears to him a true continuation of the sides.

On the basis of the general principle above enunciated, we may proceed to the explanation of a series of more complex figures. We turn to Fig. 5. Here the effect of the obtuse

FIG. 5.

angle *ACD* is to make the continuation of the line *AB* fall below the line *FG*, while the effect of the acute angle is just the reverse, but, by our corollary, the former preponderates over the latter and directs the illusion. The line *EC* adds nothing essential to the figure, for it simply introduces two angles, *ECB* and *ACE*, which reinforce the angles *ACD* and *BCD*. Likewise the line *BC* might be omitted or covered, and leave the illusion essentially unaltered. In Fig. 6 we observe a slightly

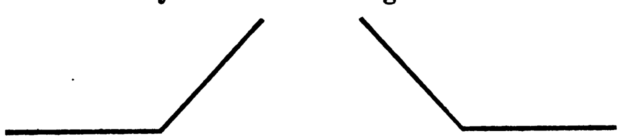


FIG. 6.

different form of the illusion, the continuation of each line appearing to run below that of the other, so that these continuations would meet at an obtuse angle. All these variations follow from the dictum that the direction of the side of an angle is deviated toward the direction of the angle.

We may further note those cases, in which the effect of each angle is counteracted by that of another, resulting in the disappearance of all illusion; this occurs when all the angles are equal, that is, are right angles. This appears in Fig. 7 and would appear equally well in any form of a rectangular cross with lines continuous with any of its arms. If we omit or cover the portions of the vertical lines below the horizontal in Fig. 7, we obtain a very instructive figure. If we observe the horizontal lines, we notice that they do not appear perfectly horizontal, but each appears to tip upwards slightly from the apex; i. e., is deviated toward the direction of the angle; so

also if we observe the vertical lines, we notice that they do not appear exactly vertical and parallel, but the right hand

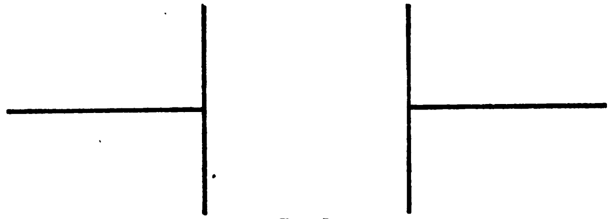


FIG. 7.

line tips slightly toward the right, the left hand line toward the left; i. e., they are likewise deviated toward the directions of their angles. This tendency of the sides of an angle to be deviated toward the direction of the angle, may result not only

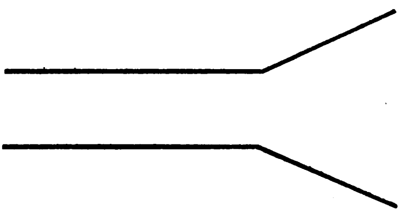


FIG. 8.

in making continuous lines appear discontinuous, but also in making parallel lines appear to diverge from parallelism.

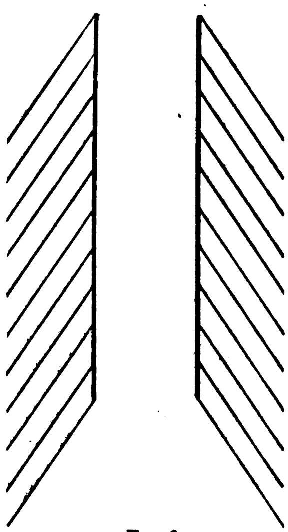


FIG. 9.

We may further illustrate the relation of divergence from continuity to divergence from parallelism by rotating the right half of Fig. 3 through 180°, and placing it under the left half. In this way we obtain Fig. 8, which shows that the same angles as readily produce slight deviation from parallelism as from continuity. To strengthen this illusion, we multiply the number of oblique lines and thus of obtuse angles. In so doing, we unavoidably introduce acute angles, but as before their effect is out-weighted by that of the obtuse angles. We thus obtain Fig. 9, in which the parallel lines diverge markedly above

and converge below. If we now carry the diagonal lines *across* the vertical ones, the illusion remains, and it is clear from our dictum that it should (v. explanation of Fig. 5.) By simply adding more main lines, we have the figure of Zöllner, with which we set out[1].

Having thus given a resumé of the series of illusions from simple to complex, we may proceed to apply our principles to the explanation of other forms of the illusion. Fig. 10 shows the illusion of discontinuity; the line *a* appears continuous

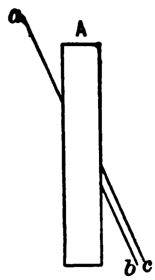


FIG. 10.

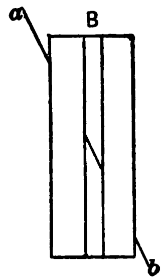


FIG. 11.

with *c*, but is so with *b;* and this is neatly emphasized in Fig. 11, in which a continuous line is deviated once in one direction and again in the opposite; the use of rectangles, instead of pairs of vertical lines, makes no essential difference. Fig. 12 presents the same illusion with the lines horizontal, the line *a* appearing continuous with *c*, while it is so with *b*. In each case the obtuse angle out-weighs the acute angle and determines the direction of the deviation. Fig. 12, when contrasted

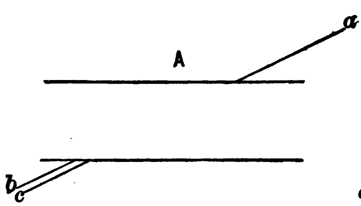


FIG. 12.

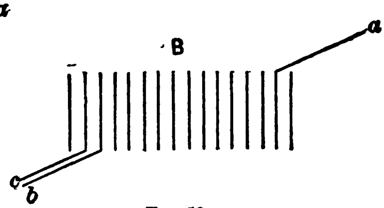


FIG. 13.

with Fig. 13, shows the effect of the position of its angles; in the former, *c* seems continuous with *a*, while *b* is really so, because the lower obtuse angle attracts the deviation of the line *c* towards itself; in the latter, the obtuse angle actually drawn between *c* and one of the vertical lines out-weighs in

[1] The oblique lines have been made shorter, but this does not add anything essentially different. It keeps the figure compact, and thus readily allows the judgment of parallelism.

effect the angle that is suggested between c and the horizontal line formed by the end-joints of the vertical lines, and thus the true continuation of a is below the apparent continuation. The divergence and convergence of the horizontal lines in Figs. 14 and 15 likewise follow from the above principles and

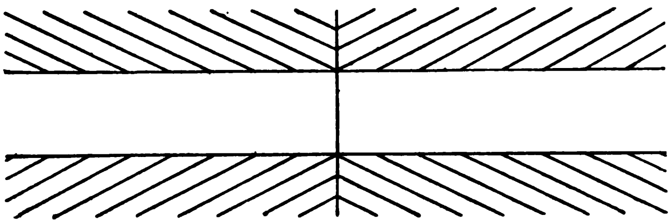


FIG. 14.

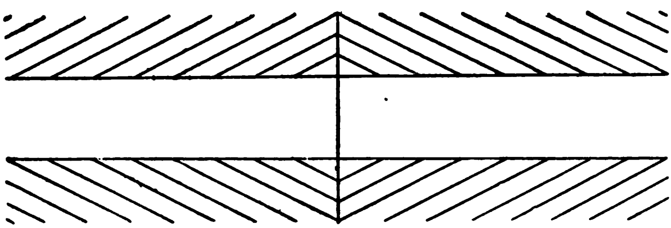


FIG. 15.

illustrate some of its more complex forms, while most complex and brilliant of all is Fig. 16. In these figures the same

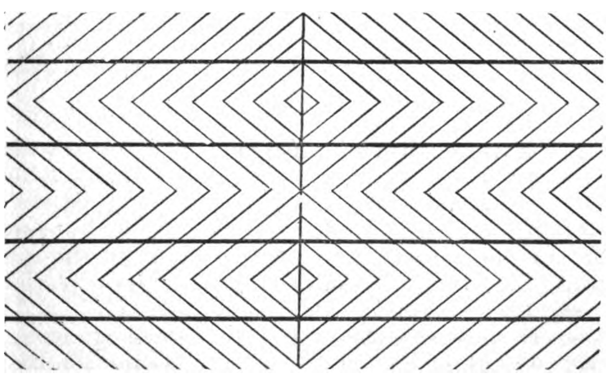


FIG. 16.

line is made to deviate in opposite directions (by oppositely directed obtuse angles) from its centre, and thus theconverg-

ence and divergence of the lines is greatly emphasized. The points at which the apparent change of direction occurs are also emphasized by cross lines. Fig. 17 adds the further

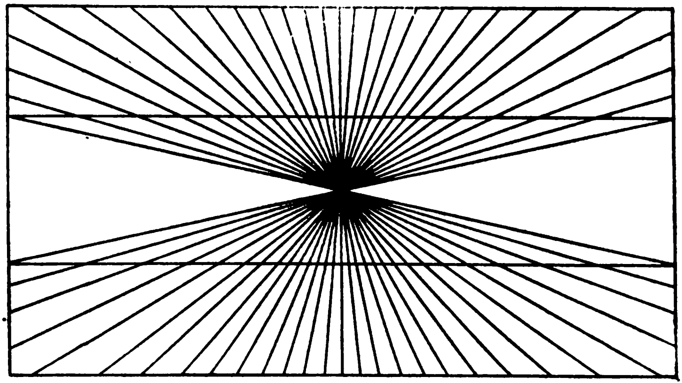


Fig. 17.

principle that the extent of the apparent deviation varies directly with the size of the angle, for as each successive angle increases (or decreases), the deviation increases (or decreases), so that the straight line becomes a line with a continuous change of direction, that is, a curve; as before, the obtuse angles are the significant ones.

Helmholtz finds a similar illusion in which motion is involved and which Prof. James thus describes (Fig. 18.)

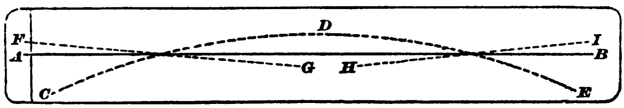


Fig. 18.

"Let A B be a line drawn on paper, C D E the tracing made over this line by the point of a compass steadily followed by the eye as it moves. As the compass point passes from C to D, the line appears to move downward; as it passes from D to E, the line appears to move upward; at the same time the whole line seems to incline itself in the direction F G during the first half of the compass's movement, and in the direction H I during the last half; the change from one inclination to another being quite distinct as the compass point passes over D." The line formed by the movement of the compass points acts as two oblique lines crossing the horizontal one. Curved lines produce the same illusion, as may be

seen in Fig. 19, by the apparent sagging of the lines at the centre. The illusion is here strengthened by the presence of several curves.[1]

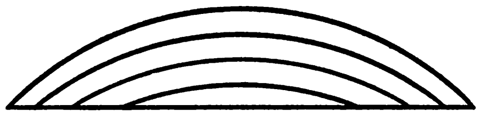


Fig. 19.

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[1] Wundt figures two illusions, which, apparently, are exceptions to our generalization, and which, accordingly, demand attention. In Fig. 20 the horizontal line appears as two lines tipping slightly downward

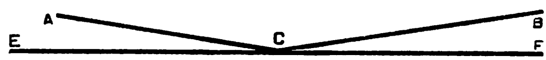

Fig. 20.

from the centre. Our first impulse would be to regard the illusion as due to the angles *ACE* and *BCF*, and we should, according to our dictum, expect the lines to tilt *upward* slightly. But remembering the greater effect of obtuse angles, we should view the figure as composed of Fig. 6, in which the two horizontal lines are approached to one another until they meet; when, by the effect of the angle *ACF*, *EC* is tipped down, and by the action of *BCE*, *CF* is tipped down. That such is really the natural way of looking at it will be evident from Fig. 14; at the centre of the upper line we have the very same arrangement of lines producing the same effect, and immediately in conjunction with the effective obtuse angles.

Wundt's next figure is more difficult to explain (Fig. 21). There the

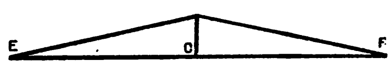

Fig. 21.

lines tip up at the ends, and there are apparently no angles which would make them do so. We have come to the view that this figure is a modification of Fig. 22. Here the obtuse angles are present and determine the

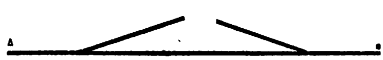

Fig. 22.

illusion. The oblique lines need not join to produce the effect, and the short vertical line, as in the other cases, simply brings out the point at which its change of direction takes place. We judge the tipping of the lines by reference to a horizontal which we carry with us or have suggested by the line of the page or the many horizontal objects we behold. We must likewise infer that the tipping in Fig. 21 is due to the obtuse angle formed by one side really present, and another suggested only by the continuation of *CE* and *CF*. The following considerations may serve to remove the artificiality of this explanation: (1) we do frequently judge by reference to imagined lines; e. g. our horizontal and vertical; (2) we use suggested lines in illusions; (3) the centre of Fig. 5 above presents Fig. 21 as the end term of a series, and in conjunction with the effective obtuse angle; (4) the illusion increases as this imagined obtuse angle increases, but decreases as the real acute angle increases.

The last illusion to which we shall attempt to apply our dictum is the familiar one whereby a square enclosed within a circle seems to bend in the circle at the four points of contact. That the circle contributes no essential part to the illusion can be shown in Fig. 23, in which the rectangle in-

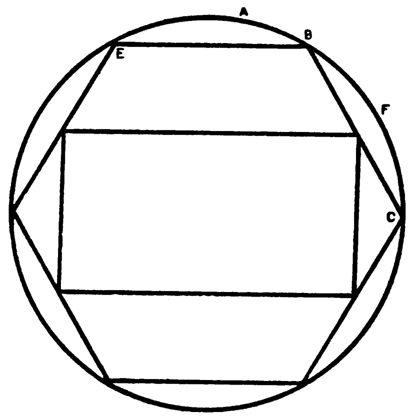

FIG. 23.

scribed in the hexagon bends in the sides of the hexagon at the points of contact. Furthermore, the fact that the hexagon shows the illusion even better than the square, suggests that the larger angle is the more effective. This illusion we regard as a complicated form of Fig. 20 inverted. From our former explanation it is already clear why we should have a large obtuse angle at the point of contact instead of the actual continuous line. The portion of the curve adjoining the point of contact acts as a straight line, as it also does in Fig. 19. The effective angles are thus the angles of which *ABC* and *DBF* are types. The application of our generalization to forms of illusion not taken into account in the formulation is a very gratifying index of its value.

We turn finally to a brief account of the literature of the topic. Zöllner accidentally noticed the illusion on a pattern designed for a print for dress goods. He established the following points: (1) the illusion is greatest when the main parallel lines are inclined 45° to the horizontal; (2) the illusion disappears when viewed at a slight angle, as by holding one corner of the figure up to the eye; (3) it vanishes when held too far from the eyes to clearly see the cross striations; (4) the illusion is as good for one eye as for two; (5) the

strength of the illusion varies with the inclination of the oblique to the vertical lines. In later studies he determined that (6) the angle between oblique and vertical lines at which the illusion is greatest is 30 degrees; (7) the illusion appears under the illumination of an electric spark quite as strongly as otherwise; (8) viewing it through red glass weakens it.

He also answers criticisms by Helmholtz and Hering. His explanation is curious and in its details unintelligible. He draws an analogy between these and illusions of motion and makes all depend on the view that it takes less time and is easier to infer divergence or convergence than parallelism.[1] Why the illusion should vary with the angle, under this theory, he does not explain; the fact that it is greatest at 45° he regards as the result of less visual experience in oblique directions. Apart from the fact that this theory does not explain and is not applicable to many of the figures, it can be experimentally disproved by a figure similar to Fig. 1 but with the lines actually inclined but apparently parallel, as suggested by Hering. Here really divergent lines all seem parallel, showing that the illusion does not consist of the inference of parallelism or non-parallelism, but of a certain angular distortion of the real relations of lines.

Hering *(Beiträge zur Physiologie*, 1861, pp. 69—80) added several of the figures above noticed (Figs. 14, 15, 19). He bases his explanation upon the curvature of the retina and the resulting difference in the retinal images of arcs and circles. He figures this explanation for the square enclosed in a circle and applies it to the rest. He criticises Zöllner and dismisses the fact that the illusion is strongest in oblique directions, as irrelevant. In a later article (Hermann, III., p. 373), he brings in the additional statement that acute angles appear relatively too large and obtuse ones too small.

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[1] It is well to note that Poggendorf called Zöllner's attention to a further illusion in his figure. This was printed in deep black lines, and the two parts of the oblique line crossing it seemed not quite continuous; *i. e.*, the illusion of Fig. 10, with a broad black line for the rectangle. Zöllner regarded this as unrelated to the other and accredited it to astigmatism.

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He also prints a figure (Fig. 24) based upon the fact of greater illusion in oblique directions. This figure, as Aubert has pointed out clearly, refutes Hering's theory, for it shows

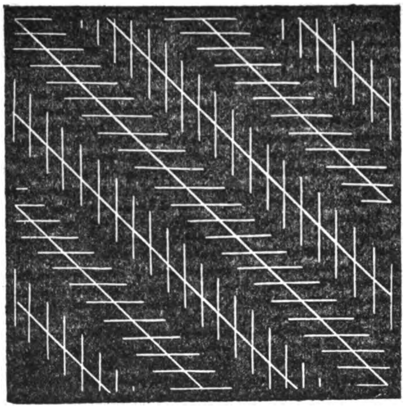

FIG. 24.

a variation in the strength of the illusion, while the retinal image remains the same.[1]

Aubert *(Physiologie der Netzhaut,* 1865, pp. 270—272) confines his attention to a notice of the results and views of others, closing with the sentence: "I am unable to give any explanation of Zöllner's illusion." In a later work, *(Physiologische Optik,* 1876, pp. 629—631), he practically repeats his former statements, and mentions that Volkmann explained it by an apparent alteration of the plane in which the oblique lines appeared; *i. e.*, they appeared in a plane inclined to that of the paper, and the inclination of the long parallel lines to this plane appears as inclination toward one another.

Classen *(Physiologie des Gesichtssinns)* after disagreeing with all previous theories, gives his own explanation in these words: "Now the cause of the illusion is clear: in recognizing the directions of the converging and diverging oblique lines, we judge them by their relations to the vertical ones. These recede from the oblique lines where they diverge, and approach them where they converge; and thus the direction

[1] Kundt (cited by Aubert) attempted to get an experimental proof of Hering's view, but his results at close distances, which alone are relevant, failed to corroborate the theory. Kundt also determined the relation between the size of the figure and the distance from the eye at which the illusion disappeared.

of the verticals is regarded as a separation toward the side of convergence and an approaching toward the side of divergence." Classen noticed that the illusion appeared as soon as a pair of parallels was crossed by a pair of oblique lines which formed an acute angle at their junction. He insists that a pair of lines of opposite direction is necessary to produce the illusion, and leads one to infer that if this were not so his theory would be disproved. This can be shown in various ways; *e. g.*, by drawing only the left half of Fig. 9 and substituting a parallel line for the right half, the illusion remains, though not so distinctly.

Lipps (*Grundthatsachen des Seelenlebens*, 1883, pp. 526—530) regards the illusion as primarily psychical; whatever parts the movements of the eyes play being determined by the attention. He says: "If we draw (Fig. 25) the line *pm*

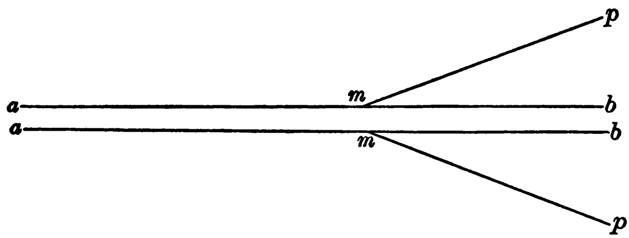

Fig. 25.

upon the line *ab*, and follow the latter with our eye, we shall, on reaching the point *m*, tend for a moment to slip off at and to follow *mp*, without distinctly realizing that we are not still on the main line. This makes us feel as if the remainder *mb* of the main line were but a little away from its original direction. The illusion is apparent in the shape of a seeming approach of the ends *bb* of the two main lines." Prof. James, whose words we have been quoting, adds: "This, to my mind, would be a more satisfactory explanation of this class of illusions than any of those given by previous authors, were it not again for what happens in the skin." Prof. James thinks that this class of illusions belongs to the field of sensation rather than of unconscious inference.

Hoppe (*Physiologische Optik*, 1881, pp. 73—83) gives a careful and critical digest of the views of others, which he finds it difficult to understand. His own is no less so but seems to be that our eyes and our attention are drawn to those lines that do most to fill out space, and that we run out the oblique lines until they meet; from this imagined point of junction the real parallels look divergent. When we carefully fixate the parallel lines, the illusion is avoided. Or

again, "we follow the *filled* middle space according to the course of the oblique lines and *neglect* the black parallel straight lines; or, because the latter are thus less noticed and viewed from a greater distance, we strengthen the appearance of their separation by the indirect view we obtain of them." The illusion is not retinal, because it vanishes in the after image; it is intellectual in origin. It is difficult to see how Lipps's view would be expressed so as to apply to Fig. 2, or why the illusion should disappear in Fig. 7. Hoppe's view is open to the same objection as Classen's and is refuted by the same figures.

Wundt *(Physiologische Psychologie,* 3d ed., II., pp. 124—132) although bringing in other factors as well, makes his main argument rest upon the view that we tend to overestimate acute and underestimate obtuse angles. He gives no proof of this fact, if fact it be, nor explains in what manner the error appears. He seems to mean that, in judging of the direction of the sides of an angle, we view acute angles as larger than they really are. If this be so, there must be some angle at which the illusion disappears, and this would seem to be the right angle; however, we get the illusion with right angles. Again, in Fig. 5 and many similar figures that we could construct, the acute angle judged by the same means would appear to be smaller than it really is, and in many respects acute and obtuse angles are affected alike. In common with others, Wundt regards the increase of the error at 45° as due to a less exact visual experience.

Pisco *(Licht und Farbe,* 2d ed., 1876, p. 268) gives no explanation, but adds the beautiful Fig. 16.

Helmholtz *(Physiolog. Optik,* pp. 564–574) presents a peculiar view of the subject. He begins with the illusion of the deviation in direction of the two parts of an oblique line separated by a rectangle and regards the particular cause of this illusion to be the curving in of the oblique lines as they meet the sides of the rectangle or heavy vertical lines. Moreover, this is especially true of small figures, in which as a whole the illusion is more marked. This deviation, then, at least in small figures, is due to irradiation. He supplements this explanation with one that will apply to large figures and to Zöllner's illusion. He says: "We may consider these illusions as new examples of the law above indicated, according to which acute angles, being small in size and clearly limited, appear in general as too large when compared with right or obtuse angles." Moreover, movement plays a large part in at least some of the figures, and in these the illusion disappears under precise fixation and the electric spark. This effect of movement is illustrated by the instance

cited above and leads to a sort of contrast whereby a clearer difference seems a larger one. Besides the general objection that so many principles are brought in to explain facts so clearly belonging to one sphere, and the further objections which have already been advanced against the alleged over-estimation of acute angles, several detailed criticisms might be made. In the first place, Helmholtz has not shown that small figures present the illusion better than large ones; in his figures he has drawn less than half as large an acute angle in the small figure as in the large one, and this is the cause of the difference he observes. Regarding the alleged curvature of the lines, it is difficult to see it; and it, as well as the possibility of irradiation, may be eliminated by drawing all the lines light and not allowing the oblique line to quite meet the vertical ones—under such circumstances the illusion persists. Helmholtz's chief argument for the effect of fixation is drawn from the heavily-drawn form of Zöllner's figure, in which he looks at the white bands with oblique lines running out like the feathers on an arrow, and *sees them parallel:* but this is precisely what *must occur* from the position of the angles, the effect of each angle being compensated by another. The two modes of drawing the figure make two figures of it. The arguments from the electric spark experiments are certainly questionable both in fact and inference, and it must be admitted that the entire treatment is unsatisfactory.

It will be seen that the field we have entered is a very complex one, and that a most important problem—Why do we deviate the sides of an angle toward the direction of an angle?—remains to be solved. How far does this depend on eye movements, how far upon inference, and the like? The chief defect of former attempted explanations seems to us to consist in theorizing upon too limited a range of facts. What is true of one group of figures fails to apply to others. Before an explanation can be satisfactory we must know precisely what it is that we are to explain, and this necessitates a correct and comprehensive generalization of the facts: this it is that we have attempted to supply.

Our study of these illusions leads us to regard them as essentially psychological in origin; they are illusions of judgment and not of sensation. Furthermore, we would regard them as an outcome of the general principle that we are prone to judge relatively rather than absolutely; that our perceptions differ according to their environment; that a sense impression is not the same when presented alone and when in connection with other related sense-impressions. A line presented

by itself is a different object from a line as a part of an angle or of a figure. However much we desire to consider the line independently of the angle, we are unable to do so. We have the direction of an angle and the direction of the lines that form the angle, and we are unable to consider the latter absolutely without reference to the former. The more nearly the directions of the angle and of the sides coincide, i. e. the smaller the angle, the smaller will be the error induced by this relative mode of viewing the lines. The whole series of illusions would thus be subsumed under the law of contrast or better of relativity; and the different variations and degrees of the illusion would find their explanations in the readiness with which they suggest and enforce misleading comparisons.

In order to exhibit a type of illusions most readily explicable from this point of view, as well as to exemplify the suggestiveness of the latter, we will consider an allied group of visual illusions.

Just as the presence of angles modifies our judgment of the *directions* of their sides, so too, the angles will modify the apparent *lengths* of lines. This form of contrast is most strikingly exhibited in Fig. 26, and best by comparing I and IV, i. e. cover up II and III. It seems almost incredible that the horizontal portions of I and IV are of equal length, and yet such is the case. II and III supply the intermediate steps, and in comparing the four figures the horizontal portions seem to become successively shorter from I to IV, while,

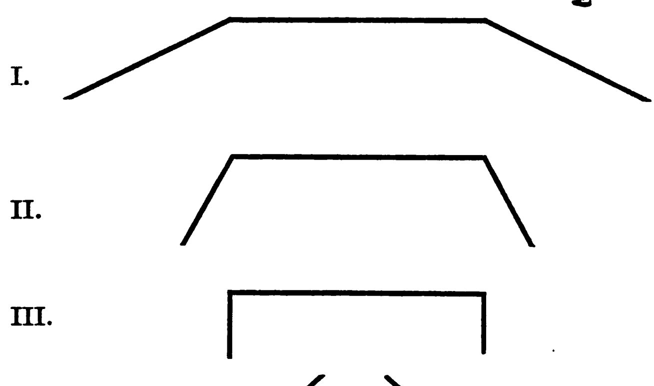

FIG. 26.

in reality, they are all one length. Here, again, the greater the angles formed at the extremities, the greater the apparent

length of the line; and thus the constrast is greatest between the very obtuse and the acute angles. Other factors contribute to the illusions; e. g. the positions of the figures, the juxtaposition of certain lines, the distance between the figures, and the like. The illusion persists if the horizontal lines be omitted, and we judge the spaces between the oblique lines. It also shows very well by cutting the figures out of paper either as they are or as truncated pyramids (by joining the ends of the oblique lines by a line parallel to the horizontal one), and viewing them against an appropriate back-ground.

We may also be tempted to judge of two areas by their juxtaposed lines, thus regarding one of two equal areas as larger than the other. This is shown in Fig. 27, which also

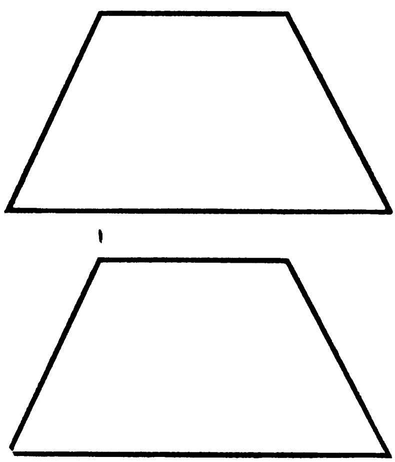

FIG. 27.

shows very well when the figures are cut out and moved about to assume various positions. The upper figure seems larger, because its long side is brought into contrast with the shorter side of the other figure. Similarly, a square resting on a corner seems larger than one resting on a side, because we then contrast the diagonal with the side. Fig. 28 on the following page presents another illustration of the same principles; the lower figure seems to be distinctly the larger, and the contrast is emphasized because it is thrown entirely to one side of the figure. In judging areas, we cannot avoid taking into account the lengths of the lines by which the areas are limited, and a contrast in the lengths of these is carried over to the comparision of the areas. We judge relatively even when we most desire to judge absolutely. Relative distinctions and

the perception of relation seem to be more natural and significant than absolute ones. We cannot view the part as unrelated to the whole. This is a widely applicable princi-

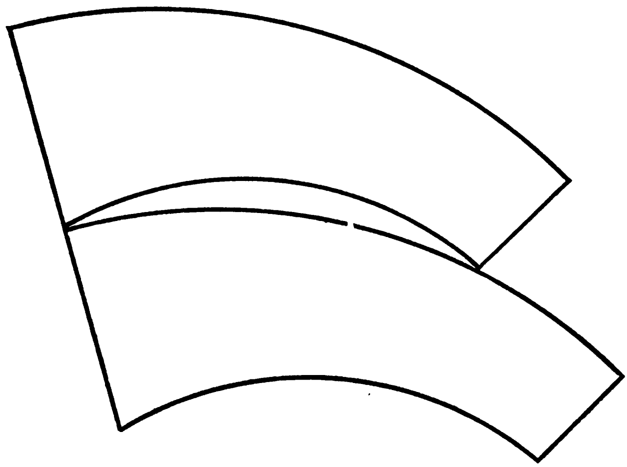

FIG. 28.

ple and is suggested as a convenient guiding principle by which the study of such illusions of sense may be profitably directed.

A STUDY OF INVOLUNTARY MOVEMENTS.

(With the assistance of HELEN WEST.)

The dictum that thought is repressed action most readily finds illustration in conditions of the nervous system varying somewhat from the normal. It is easier to detect the action of not definitely recognized laws in extreme forms than in average ones. The modern view of morbid action, however, emphasizes the close relation of the abnormal to the normal; there exists in the latter in germ and to a limited extent, what is full grown and characteristic in the former. If, under great excitement and extreme fascination of the attention and in favorably constituted individuals, the involuntary movements are pronounced, the rudiments of these movements should be demonstrable in the average individual under normal conditions. For this purpose delicate apparatus may be requisite, and a variable amount of success is to be expected. The question of apparatus is of importance, and our present study aims to do little more than describe the apparatus and illustrate what results may be obtained therewith.

The apparatus is so simple that a brief description will doubtless be sufficient to convey a clear idea of its mode of action. There is first a piece of plate glass (see Fig. 29)

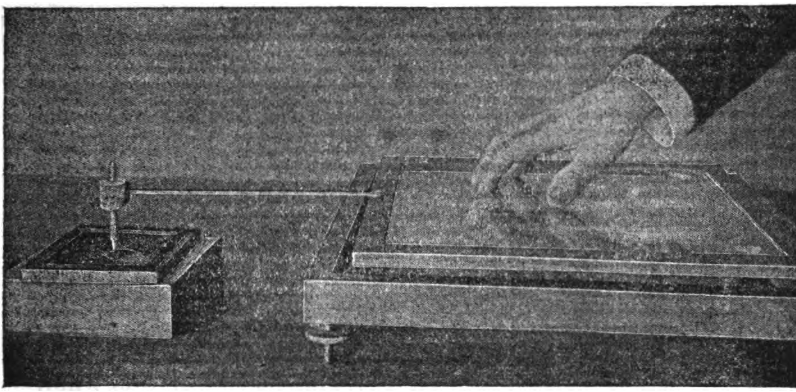

FIG. 29.

fiifteen inches square, resting in a stout wooden frame; this frame is mounted on three adjustable brass legs, raising it an inch or so from the table. By means of the screw-adjustments of the legs, the plate glass is brought into exact level. Three brass balls, which must be very perfectly turned and polished spheres, about three-fourths of an inch in diameter, are placed in the form of a triangle upon the plate; upon these balls rests a very light crystal-plate glass, fourteen inches square, mounted in a light wooden frame. On the upper surface of this plate is placed a piece of paper to hide the balls, and on the paper we lightly rest the finger-tips of our hand. It is almost impossible to keep the plate from all motion for more than a few seconds; the slightest movement of the hand slides the upper plate upon the balls. To maintain the apparatus in working order it is necessary to keep the glass and balls well polished by rubbing with a cloth and a little oil.

The recording of the movement is equally simple. To the light frame is attached a slender rod about ten inches long, bearing at its end a cork; piercing this cork is a small glass tube and in the tube there is a glass rod snugly fitting the tube and drawn to a fine point. The point of the rod traces the movement of the hand with great accuracy, and, not being rigidly fixed, can accommodate itself to all irregularities of movement or of the writing surface. A piece of smoked paper stretched over a glass plate, upon which a record is

made, and a large screen to prevent the subject from seeing the record, complete the apparatus. This apparatus enables us to record all movement in the horizontal plane, and, inasmuch as its chief purpose is to write slight involuntary movements, we have given it the name of the *automatograph* and may speak of such a record as an *automatogram*.

The type of an experiment is the following. The subject places the finger-tips of his extended right hand upon the glass; he is told to hold the arm still and pay no attention to it. He is asked to read some lines or colors, or to count the beats of a metronome; this naturally engages his attention. When all is in readiness the operator drops the glass rod into the tube, and the record begins. When the subject has been occupied in this way for a minute or so, we have, as a rule, a very clear record of the direction of his attention in the automatogram.

In order to have a test by which to compare the relative sensibility of different persons for movements of this kind, we arranged to have a number of persons go through a series of tests, a typical result of each of which will be figured. Each experiment occupied from about three-fourths to two minutes, and when possible we noted the progress of the record for each 15 or 30 seconds.

A series of patches of color 5x20 mm. were placed in horizontal rows on a vertical wall about ten feet distant. The subject was required to read aloud the names of the colors. The general tendency is for the hand involuntarily to move toward the colors with a variable degree of constancy, rapidity and directness. An average result is shown in Fig. 30. We have another record, lasting but 45 seconds, but covering 6½ inches, which in extent and directness is the most remarkable of our records. The appearance of the line is similar to that of Fig. 30, but with several points at which the line is almost directly toward the colors.

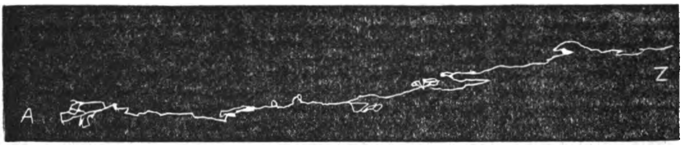

FIG. 30. ⟶

Reading colors. Time, 95 seconds. A indicates the beginning, and Z the end, of each record. The arrow everywhere indicates the direction in which the object attended to was situated. When the numbers 1, 2, 3, 4, occur, they indicate the point of the record at 15, 30, 45 and 60 seconds after the beginning of the record.

On two occasions the subject who gave us this striking record evidenced the action of the attention in another, equally striking way. There were three rows of colors which

were read; the first one from left to right, the second from right to left, and the third from left to right; the involuntary movements correspond to the movement of the attention, as is vividly shown in Fig. 31.

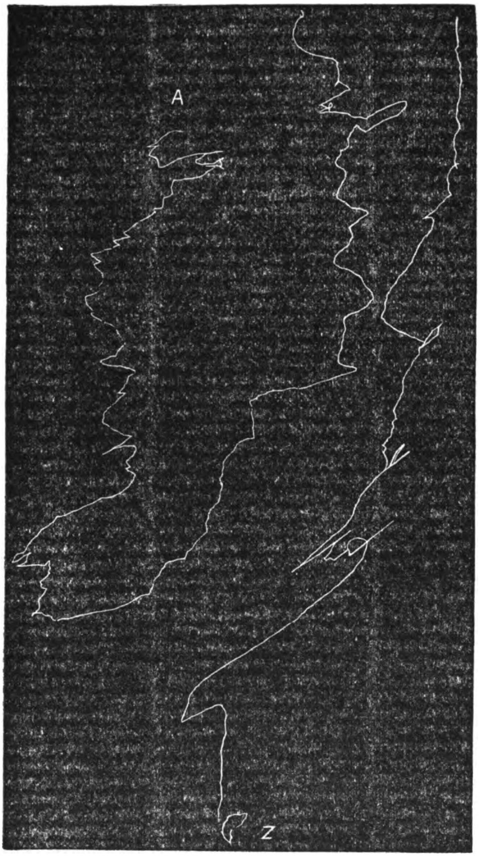

FIG. 31. ⟶

Reading three rows of colors; the movements closely following the attention. Time, 90 seconds.

These are certainly striking proofs of the ease with which, in sensitive subjects, the hand involuntarily follows the movements of the eye.

A second test consists in substituting the reading of a

printed page for the colors; the results are quite similar. Fig. 32 represents a typical result.

FIG. 32. ⟶
Reading from a printed page. Time, 45 seconds.

We next pass on to cases in which the attention is directed to sounds. We set going a metronome, and ensured the subject's attention to it by having him count the beats. The usual rate was 140 strokes per minute. Here, again, we find two types of involuntary movement: the one a moving toward the sound, represented in Fig. 33; the other a keeping time with the beats, not accurately at all, but in a general way, as is shown in Fig. 34. When we consider

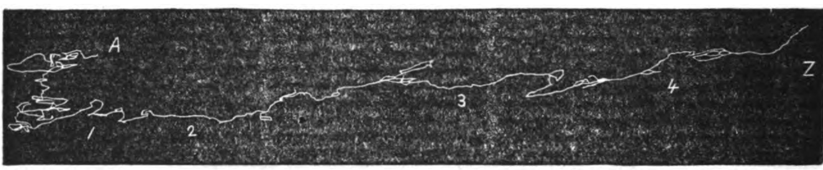

FIG. 33. ⟶
Counting strokes of metronome at 140 per minute. Time, 70 seconds. Also illustrates slight hesitation before movement towards metronome begins.

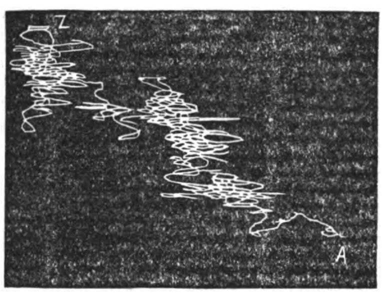

FIG. 34. ⟵
Counting strokes of metronome. Shows movements to and fro with the strokes.

how strong is the tendency to keep time to enlivening music, it will not surprise us that we are able to record these slighter and more unconscious movements to simple time beats. We frequently performed this experiment by placing

the metronome first in front of, and then behind the subject, and the contrast between the direction of the lines is, as a rule, quite striking.

We recorded a similar experiment for sight, by substituting for the metronome a silently swinging pendulum, the oscillations of which were to be counted. Again we observe the two kinds of records, the second, as before, being considerably less frequent than the first. These are given in Fig. 35 and Fig. 36. A pair of records derived from this form of ex-

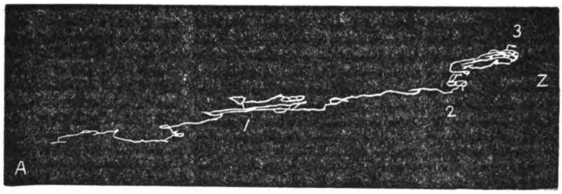

FIG. 35. ⟶
Counting oscillations of a pendulum. Time, 45 seconds.

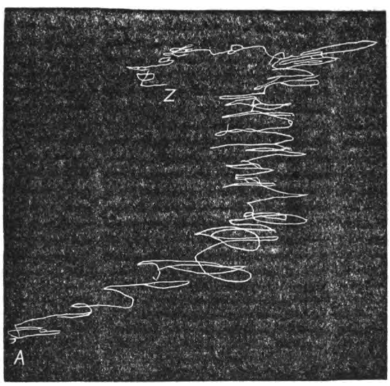

FIG. 36. ⟶
Counting pendulum oscillations; shows movements to and fro with the oscillations. Time, 80 seconds.

periment well illustrates the extremes of rate of movement: one subject moved 11 inches in two minutes, another 1$\frac{3}{8}$ inch in the same time, though in both cases the motion was regularly toward the point of attention, the swinging pendulum.

Our next experiment approximates closely that of the muscle reader. We directed the subject to hide a knife at some part of the room not near the center, and immediately thereupon took a record upon the automatograph, the subject

thinking of the knife. In some cases this experiment was unusually successful, in others fairly so; the direction of movement usually closely approximated the direction in which the knife lay. Fig. 37 represents a fair result. A

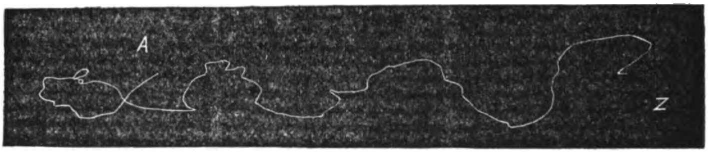

FIG. 37. ⟶
Thinking of hidden object. Time, 30 seconds.

quite similar experiment consists in directing the attention to some prominent building or locality in the neighborhood, not by actually looking toward the place, but by voluntarily thinking of it. We have many very excellent examples of such records. Fig. 38 will serve as a type of the more successful ones.

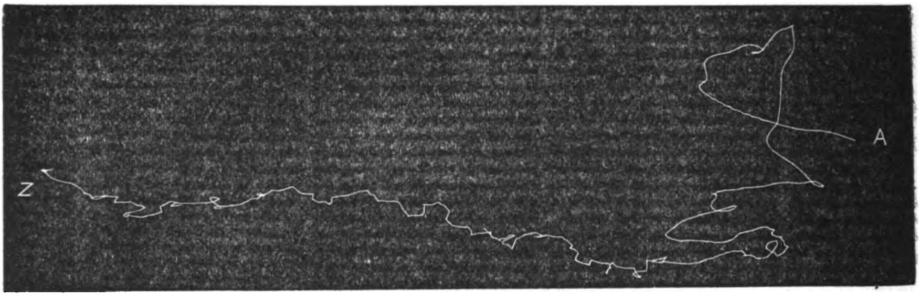

FIG. 38. ⟵
Thinking of a building in the direction of A to Z. Time, 120 seconds.

This does not exhaust the methods of attracting the attention but it illustrates our chief modes. Reading to a person from different parts of the room is often successful. Quite an interesting form consists in having the subject's attention change in the course of an experiment to different localities, as by having him read from a book carried about by an assistant. Such a result is shown in Fig. 39, in which we have an irregular figure closing in upon itself and clearly indicating the circular movement of the book.

We often succeeded in distracting a subject's attention by a noise in another portion of the room, the hand moving toward the source of the noise. We also recorded the involuntary start that occurred when a ball was suddenly dropped upon the floor.

The figures given will sufficiently illustrate the nature of

the results obtainable with the aid of our automatograph and it remains only to notice a few general points regarding them.

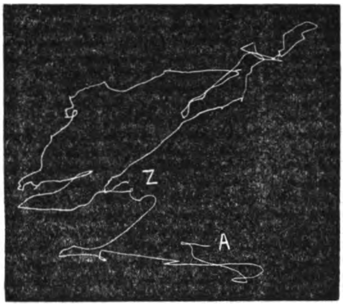

Fig. 39. Reading from a page moved about in a circle.

It would be interesing to determine by this method the relative degrees of muscular accompaniment for these different kinds of attention; but our methods are not as yet sufficiently refined to solve this problem; the result seems to vary with individuals and with the sense organ engaged. As a preliminary result it may be worth recording that a number of measurements yielded an average rate of movement of about two inches to the minute toward the object thought of.

Of great importance is the nature of the individual differences in these experiments. Our normal experience would naturally anticipate a difference about as characteristic at least as that of hand-writing. Any minute discussion of the point would be obviously premature, but in general it seems possible to arrange these differences in types. We should distinguish between those who move rapidly and directly, and those who move slowly and circuitously; between those in whom the movement quite exactly follows the line of attention, and those in whom it does so only approximately or irregularly. Instances of such distinctions have already been indicated.

We add Fig. 40, which may be contrasted, in regard to the

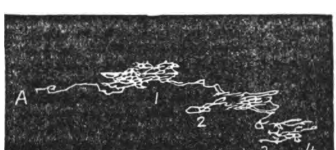

FIG. 40.

Counting oscillations of a pendulum. 1, 2, 3, 4 indicate the points 30, 60, 90 and 120 seconds after the start. Time, 120 seconds. Illustrates small and indirect type of movement.

character of the curve with Fig. 31; the latter shows directness of movement and great extent, the tracing rarely becoming confused by the hesitancy of the subject, while, in Fig. 40, the movement is slow and the record involved by continual retracings of the path of movement. Another important distinction relates to the time at which the most significant movements occur, and mainly whether the first impulse is toward the object of attention followed by much hesitation of movement; or whether at first there is little movement followed, after fatigue, by the movement determined by the attention. We have many more of the former type than of the latter; one of the former is presented in Fig. 41. Figs. 33

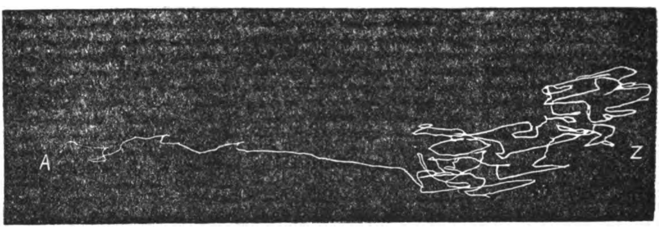

FIG. 41. ⟶

Counting beats of metronome. Illustrates first impulse toward object of attention followed by hesitation. Time, 90 seconds.

and 38 partly illustrate the reverse tendency. We might further distinguish between those subjects who show the direction at each portion of the tracing and those who show it only here and there.

These types may possibly suggest what kinds of involuntary movements best subserve the purposes of the muscle reader; all alike illustrate the general line of tendencies which he utilizes.

A very natural query relates to the possible influence of the position of the arm and body, and also of such other factors as the pulse and respiration upon the character of the tracing. The main distinction in regard to the position is whether the arm is (1) held straight out from the shoulder in a line with the trunk, or (2) at 90° from this position, or (3) in an intermediate position. In the first of these, movements toward the front are obviously easier to make than movements toward the rear, and in both (1) and (2) movements toward the body are easier than those away from the body. By changing the position of the object to which the attention was given, we could thus favor or interfere with the tendency to involuntary movement in certain directions. In our experiments, we allowed the subject to assume a natural and comfortable position, which was usually an intermediate one,

with the arm not fully extended. This position allows movement in all directions, though it is still true that movements toward the front and toward the body are favored above movements toward the rear and away from the body. The direction of the attention is thus sometimes partially obliterated in some subjects, but in most of them it appears in spite of this tendency. This factor deserves a more special investigation. While respiration may have some effect, we are inclined to regard it as very small, and the pulse as not entering into consideration at all; for, in order to get the tracing of pulse and respiration (by other apparatus) with equal distinctness, we had to magnify them very considerably.

The question of the precise significance of these movements is largely dependent in the testimony of the subjects. While there are individual differences in this as in all other respects, the consensus of the verdicts might be thus expressed: at times we become aware that our hand has moved, but rarely of the direction of its movement; the movements are sometimes unconscious but always involuntary, there is often great surprise at the result. The one objective test we could apply was to intentionally simulate these movements and the result was measurably different from the genuine involuntary record.

It is hardly necessary to enlarge upon the bearings of these experiments upon the processes of muscle reading and kindred phenomena. They indicate the close connection of mind and muscle, and in demonstrating the extent of recordable automatic movements, suggest the many other and subtler means by which we may give to others some notion of what is going on in our own minds.

Observations on the Absence of the Sense of Smell.

(With the assistance of Theodore Kronshage.)

The subject of our observations is a Mr. E., aged 21 years, a student of this University, who is deprived of all the sense of smell. The defect is probably congenital and of nervous origin. As Mr. E's. knowledge of his defect was denied from such occasions as would occur in every- day life, our first step was to test the degree and extent of the anosmia. We approached various substances to his nose asking him to inhale them and report the result; we tried in this way strong liquid solutions of wintergreen, bitter almonds, ether, alcohol, ammonia, cinnamon, camphor, etc. Camphor produced very slight if any sensation and the same is true of wintergreen and cinnamon. Bitter almonds, ether, and most markedly ammonia, produced a sharp, more or less stinging sensation

in the nose. Alcohol was described as sweetish like perfumes. We next tried several substances in pairs to determine how far, when first told which was which (not by name but by calling one *A* and the other *B*), he could distinguish between them, and, as a check against unconscious bias, experiments in which one of the pair was distilled water were introduced. This precaution was quite necessary, for it happened that when bitter almonds and water were compared in this way he mistook them three times in five trials, though professing to get some sensation from the bitter almonds when presented alone. The water, however, was frequently recognized by its entire absence of any sensation of smell. Such distinctions therefore as are perceived by him are by no means altogether clear. With the pair, ether and water, eleven trials resulted in eleven correct answers, the point of distinction being that "ether opens up the throat like peppermint." With wintergreen and bitter almond, the latter yielding the distinctive effect, there were only 3 errors in 18 trials. With ether and ammonia both giving decided sensations but of somewhat different nature there were 2 errors in 8 cases; this may however have been due to over stimulation, as the substances used were so strong that neither of us could take them to the nose with comfort. Ammonia was described as immediately affecting the nose, ether as going back to the throat and affecting it. With wintergreen and cinnamon, neither yielding any definite sensation the result proved to be mere guesswork, and the same is true of cinnamon and camphor.

Inasmuch as the sensation arising from the inhalation of alcohol was described as similar to that of perfumes (alcohol being an ingredient of these) we ascertained how far the presence or absence of alcohol could serve as a means of confusion or indentification of substances. We made a strong solution of wintergreen and of cinnamon in alcohol, and from each of these Mr. E. obtained a similar but pronounced effect. The attempt to distinguish between the two, however, resulted in as many failures as successes. We next compared pure alcohol with wintergreen dissolved in alcohol, and no difference except of intensity was observed. To complete the proof we made a solution of wintergreen in water and in alcohol. The latter gave a distinct sensation, the former almost nothing. In all the eight trials the two were correctly distinguished.

The next point tested was whether distinctions of intensity within a perceived range of sensations were obtained. We tried strong and weak alcohol with the result that in all cases (eight) they were correctly distinguished from one

another; the sensation was described as a "sweetish taste in the mouth."

In the above results indications were given that Mr. E. was less than normally sensitive to irritants. To measure this difference we determined how many drops of very strong ammonia must be added to 100 cc. of water (1) to produce a sensation, (2) to make it objectionably strong. We obtained the characteristic effect with but one drop in 200 cc. and even one in 300 cc.; while Mr. E. needed 2 drops in 100 cc. Eight drops in 100 cc. made it very objectionable to us, but he said it was like some perfumes, and it took 23 to 25 drops to produce an objectionably strong sensation.

We next tested the sense of taste. A preliminary survey served to show that the sense was present and presumably in a normal degree. To complete the test we compared his taste with ours for sugar, acetic acid and quinine. We found about the same measure of sensitiveness for Mr. E. and for us; and found nothing differing from the normal in any respect.

We proceeded to investigate those mixtures of smells and tastes, which make up most of the sensations obtained during eating. We took the ordinary flavoring syrups of commerce, lemon, vanilla, currant, orange, strawberry and raspberry. From all these Mr. E. obtained only a general sweetish sensation with no distinction between them except from the lemon which was in the main distinguished by the mixture of sweet and sour. He could in part tell them by their different degrees of sweetness, but when presented in proportions in which they seemed to us equally sweet all distinctions were impossible to him. The tests showed as many wrong as right answers. It so happened that some of these substances fermented and these he could at once detect as different from the others and also the more fermented ones from the less so. A series of candies with some of the above flavors yielded corroborating results. It should be understood that all these substances were tasted.

Next, a series of spices was tried with the following results:

Mustard; a sharp sensation on the end of the tongue; not recognized.

Pepper; same effect but stronger.

Coffee; not recognized, a slight taste.

Cinnamon; recognized, sweet and sharp.

Broma; sweet.

Cloves; recognized, taste distinct but not describable.

Thyme; sharp, bitter, something like cloves.

Tea; no effect at all.

Anise; sharp, bitter, unpleasant.
Caraway Seed; mild, sweetish, and salt.
Ginger; not recognized, burns.
Mustard Seed; burns decidedly.
Citron; recognized by its feeling on the tongue, sweet.

In brief some were recognized by secondary qualities, but those that we recognize by flavor were not differentiated. A separate series was tried with tea and coffee, and one with ginger and cloves. Neither of either pair was distinguished from the other; the latter were both called sharp but with no distinction between them.

We also tested Mr. E.'s temperature sense, at about 15°, 30° and 60° R. At all these points his sensibility was as good as ours, differences of 1° being everywhere recognized. The test was made by taking a mouthful of water heated to the required temperature and then throwing it out.

The great importance of these observations lies in the analysis they enable us to make of the complex of sensations obtained in the mouth and nose. In Mr. E.'s case taste is normal, the temperature sense is normal, the tactile sensibility is present (though as far as irritants are concerned, to a diminished extent,) while smell alone is absent. Accordingly we may conclude that such distinctions as Mr. E. fails to make are in us due to the sense of smell, and such as he makes are due to other senses. *The results conclusively show that a great many of the mouth-sensations, which we ordinarily speak of as tastes, are really due to smell.* The distinctions between tea and coffee, between all the various flavors that make the difference between candies and sugar, between various syrups and so on—all these are lost. That the absence of marked sensations during eating should lead to a relative neglect of such sensations is natural; Mr. E. is perhaps on this account less sensitive to other mouth sensations.

Mr. E.'s defect was observed by members of his family as soon as the sense of smell could be tested. He has no recollection of ever having smelled and his family agree that he never gave evidence of such sensation. It is certain that he could not smell when a very small boy. He gets no sensations from flowers, perceives no difference in the taste of his food when afflicted with a cold, and observes no distinctions in the various kinds of sweet things of which he is fond. He perceives no distinction between tea, coffee, and hot water flavored with milk and sugar and has come to take the latter as his every-day drink.

Mr. E.'s case is especially interesting because his mother has a similar defect. Mrs. E. however at one time possessed the sense of smell and distinctly remembers the sensations

derived from odors and her use of odorous substances. She seems to have lost the sense when about 13 or 14 years of age. It is definitely established that she is the first and only one of her family or her husband's family to show this defect. She has two sons and two daughters besides Mr. E., all of whom are normal as regards the sense of smell. Some of the more typical experiments performed upon Mr. E. were also tried by Mrs. E. with strongly corroborative results. Ammonia alone had a marked effect; the same confusions were made by Mrs. E. as by her son. She is likewise deficient in distinguishing "by taste" between flavoring extracts and similar substances.

Classification Time.

(With the assistance of George W. Moorehouse, Fellow in Psychology, and Mildred Harper.)

An extremely frequent and important mental process consists in the reference of some item of knowledge to some familiar class. The assimilation of knowledge involves the appreciation of the relation of the new fact to the general body of acquired knowledge. In order to maintain in an orderly and accessible form our mental acquisitions it is necessary to view each item of information as belonging to such and such classes. Psychologists have variously analyzed this process; some express their views by picturing the mind as possessing a number of apperceptive instruments and using now one and now another of these according to the nature of the object to be assimilated, or, again, as a series of lanterns each of which has its own focus and field of illumination. However we may view the process it is clearly essential to the acquisition of knowledge, and it is strange that the study of the time-relations of this process has been hitherto so largely overlooked. The present contribution considers the time of a special form of such classification.

As a distinctive and readily studied form of such reaction we selected the reference of a common word to its grammatical class. We further limited the problem by selecting the following ten nouns, ten verbs and ten adjectives and confining our reactions to calling the proper part of speech to which one of these words belonged: house, cat, book, ship, ant, sun, lake, doll, man, girl; push, have, cut, mix, go, die, look, sit, jump, touch; tough, wet, good, blue, low, bad, high, thin, hot, black.

These words were chosen as familiar, distinctive and monosylabic representatives of their classes. The full list of words which he might be called upon to classify was always read to the subject before each kind of experiment. We reacted to a

spoken word by a spoken word. The apparatus by which this was accomplished consisted of a bit of wood held between the teeth connected with one arm of a lever the other arm of which bore a metallic point for electrical contact. A spring connected with the lever tended to pull the bit from between the teeth, and according to the adjustment to make or break an electric circuit. Both subject and observer used an instument of this kind, the instruments being so connected with the chronoscope that the release of the bit from the mouth of the observer started, and a similar action from the subject stopped it. The necessary act of separating the teeth that accompanies articulation is here taken as the point of measurement. The apparatus is fairly satisfactry, and so long as the results are used mainly for relative purposes the error involved in its use may be neglected. A perfect apparatus whereby the utterance of a word will start or stop a chronoscope is still a desideratum.

The entire process may be viewed as consisting of the following steps: (1) the hearing of the sound uttered, (2) the recognition of the word, (3) the reference of the word to its class, (4) the summoning of the term describing that class, (5) the muscular innervation accompanying the utterance of the term. In order to determine the time of the purely mental process involved in expressing the fact that a certain one of ten words is a noun, or verb, or adjective, it was necessary to measure separately the time of the mechanical steps. The simple reaction involved evidently consists of steps (1) and (5). This time for each of the three subjects we found to be 190^{σ}, 195^{σ} and 199^{σ} respectively. It is naturally somewhat long for a simple reaction because the muscular contractions by which it is signalled that the impression has been received are complicated, and because the moment at which the chronoscope starts may slightly precede that at which the soundwave reaches the subject's tympanum. In all these simple reactions both observer and subject used what seemed to be the easiest vocal utterance; it consisted of a violent expiration, the result resembling the sound *eh*.

We further need in order to measure step (3) in which we are particularly interested a process involving steps (2) and (4) as well as the simple reaction. It seemed impossible to devise any simple process of the kind, but the process of repeating a word sufficiently approximates it for our present purpose. This process clearly involves in addition to the simple reaction, the recognition of a word and the summoning and utterance of a word. The only question would be whether the summoning of a term denoting a grammatical class is of equal difficulty with the repetition of a recognized word, but

as both are very familiar and somewhat mechanical processes, their time relations can hardly be very different. The repetition time for the three subjects was as follows: 367σ, 280σ, 333σ.

The experiments in which words were referred to grammatical classes were of the following kinds: (1) the subject was to tell whether the word was a noun or verb; (2) the same distinction regarding nouns and adjectives; (3) the same distinction regarding verbs and adjectives; (4) the same distinction regarding nouns, verbs and adjectives. Experiments are grouped in sets of twenty each. In fact from 22 to 25 observations were taken and those most divergent from the average of all were discarded until 20 were left. A new average of these 20 was entered. The following table gives for each of the three subjects the average time of the several reactions together with the number of sets of which it is the average.

Subject.	Simple.	Repeat.	'Noun-Verb.'	'Noun-Adj.'	'Verb-Adj.'	Average of 'N-V,' 'N-A,' and 'V-A.'	'Noun-Verb-Adj.'	Mental Time.
J. J.	190 (15)	367 (10)	599 (18)	595 (10)	593 (10)	596	667 (11)	71
G. W. M.	195 (17)	280 (17)	628 (13)	597 (10)	679 (10)	635	678 (9)	43
M. L. H.	199 (10)	333 (11)	612 (15)	550 (10)	568 (10)	577	589 (10)	12
Average.	**195**	**327**	**613**	**581**	**613**	**602**	**645**	**42**

Combining the results of the three observers we obtain as the result the fact that with a reaction time of 195σ, and a repetition time of 327σ, it takes 603σ to determine whether one of a set of words belongs to one or the other of two grammatical classes (the mental portion of this process consuming 276σ), and that it takes 645σ to refer a word to one of three grammatical classes.

It hardly seemed worth while to calculate the mean variation of these observations, but to satisfy ourselves regarding the regularity of the results we calculated it for the three most typical sets under each kind of reaction. Expressing

the mean variation for these three sets as a percentage of the general average time for the kind of reaction, we obtain the following table.

Subject.	Simple.	Repeat.	'Noun-Verb.'	'Noun-Adj.'	'Verb-Adj.'	Average of 'N-V,' 'N-A' and 'V-A.'	'Noun-Verb-Adj.'
J. J.	8.0	8.4	11.0	10.3	9.9	10.5	9.5
G. W. M.	16.5	13.2	11.8	13.4	10.9	12.0	13.4
M. L. H.	20.9	9.7	8.2	5.6	7.6	7.0	8.3
Aver.	15.0	10.3	10.3	9.9	9.6	10.0	10.4

This table indicates a very fair degree of regularity with the exception that, markedly in the case of M. L. H., and to some extent in the case of G. W. M., the variation for the simple reaction is large. This is clearly due to the fact that experimentation began with the simple reaction alone so that this variation indicates absence of practice in reaction work.

The results are, however, effected by inequalities of practice. This is particularly true of the time for 'Noun-Verb-Adjective' distinctions which observations were made last and were therefore most benefitted by the practice gained in the former distinctions. It is probable, therefore, that the difference in time, 42σ between the two processes is too small. This appears more clearly in considering the results for each subject. For J. J., who began with most practice in this kind of observation and whose time for the three classes of distinction, 'Noun-Verb,' 'Noun-Adjective' and 'Verb-Adjective,' show greatest constancy, the difference in question is largest, 71σ. For G. W. M., who began with some practice in reaction experiments the difference is intermediate, 43σ, and would be greater were it not for the special and temporary difficulty he encountered in distinguishing verbs from adjectives, the difference between the average of the 'Noun-Verb' and 'Noun-Adjective' and the 'Noun-Verb-Adjective' being likewise 71σ. While for M. L. H., who began with no practice and showed steadily decreasing time for each successive kind of reaction attempted, the difference in question is but 12σ. It is probable then that the time

for J. J., 71σ, is a more typical result than the general average, 42σ.

The relative difficulty of the three pairs of distinctions, 'Noun-Verb,' 'Noun-Adjective' and 'Verb-Adjective' probably varies with different individuals; in the present study it is also affected by differences of practice; on the whole, however, our results favor the view that the three are of practically equal difficulty.

The increase in time in passing from two distinctions to three is an interesting illustration of the effect of the mental attitude on reaction times. The process involved is the same in both cases, to decide, for example, that *man* is a *noun*, but this decision requires more time when the word in question may belong to one of three grammatical classes than when it may belong to one of only two. Our results indicate that all of these processes are quite complicated and that their time-relations depend upon the accessibility of very familiar items of knowledge.

Regarding possible differences between the several words, they may vary with individuals; extended results would be needed to clearly show their existence. It is interesting, however, to observe that taking the average reaction of each word in all the three kinds in which it occurs we find among nouns "ship" was most quickly classified by all three subjects; another easy word was "man"; especially difficult nouns that were "doll,' "ant" and "cat"; among verbs "sit," "jump" and "go" were relatively easy, "have" and "cut" relatively difficult; among the adjectives "good" was particularly easy, "wet" and "blue" fairly so, "high" particularly difficult, "bad" and "hot" fairly so. It should be noted, too, that this difficulty may in part be due to difficulties of recognition and pronunciation.

FINDING-TIME.

(With the assistance of WINIFRED SERCOMBE and LUCY M. CHURCHILL, [MRS. FRANK T. BALDWIN].)

We have employed the term "finding-time" to denote the time occupied in the process of finding a given object within a given field; we recognize with what different facility and rapidity different persons perform such tasks, and even in the same individual the time is subject to variation. We have all experienced the difficulty of finding an object even when it is plainly in sight, and have wondered at the long time necessary to find a quotation in a volume and the like. In this process we carry with us a mental picture of the object sought and we react when the subjective corresponds to the objective picture. The ability to recognize one of a number of objects as the one desired is certainly a useful trait and

may perhaps be a convenient test of mental alertness. It is this process that we desired to study and to measure. The difficulty of finding an object varies with several factors, the most important of which may be thus summarized: (1) the number of objects amongst which one is sought; (2) the nature of the object; (3) the minuteness or complexity of the differences by which the one object is distinguished from the others; (4) the degree of probability (which may amount to certainty) that the object sought is within the given area.

In our study the objects sought were the letters of the alphabet; the method of finding them was as follows: The letters (plain capitals about 4 dioptrics or in the average 6.5 mm. square and very closely conforming to the Snellen types) were gummed on a card which was in turn fastened on a block, and were seen through square openings in a black screen. These openings, 25 in number, were 11 mm. square and were each separated by 19.5 mm. above and below and to each side from the neighboring opening; this screen was laid on a glass plate mounted in a square frame that slipped over the block and (inside) was about 15 mm. larger each way than the block. The block contained four alphabets distributed by a chance arrangement, and according as the frame was moved to the upper left hand, the upper right hand, the lower right hand, or the lower left hand corner, one or another of these alphabets was seen through the openings in the screen. The arrangement may be made clear by reference to the letters below. Here each different kind of type represents an alphabet and it will be clear from this how a simple movement was sufficient to bring to view through the openings in the screen another alphabet. In the original all the letters are of course alike, and distributed by a chance arrangement. Connected with the frame by means of two iron uprights was

C	S	X	O	R	L	T	M	O	T
F	I	R	C	O	G	A	R	C	H
I	J	P	A	H	B	B	C	F	K
G	S	U	N	S	L	I	A	E	J
Z	G	N	V	K	W	S	E	W	Z
M	F	Z	B	W	P	B	D	F	K
J	R	E	D	V	F	D	H	Y	N
D	Z	Y	U	J	E	V	X	H	V
G	Y	L	X	U	I	M	F	A	U
L	Y	X	M	T	W	K	T	N	O

a head piece similar to that of a stereoscope against which the subject rested his head and through two openings in which he viewed the letters. Across these openings is a hard-rubber flap which may be quickly withdrawn by bringing into action a strong spring. As this flap opens it closes an electric circuit and thus starts the chronoscope.[1]

An observation was conducted as follows: the frame is set for a certain alphabet; the operator announces the letter to be found (this also serves as a signal) and shortly thereafter he pulls a cord releasing the spring and allowing the subject a view of the letters. As soon as the desired letter is seen the subject presses a key and stops the chronoscope. To test whether the subject knows where the letter is situated he keeps a record of each answer. The positions were indicated by lettering the double rows A, B, C, D, E, and the columns 1, 2, 3, 4, 5, so that A1 would indicate the upper left-hand corner, D5 the lower right hand corner and C3 the centre letter. In the first experiments 25 letters were thus shown (Q was omitted), but this could be reduced to a four-square (16 letters) by covering over either the row A or E together with either column 1 or 5. Throughout the experiments except when distinctly stated otherwise, the subject was assured that the letter sought was present.

The following table represents our average results for the three observers separately and together.

	Finding one of 25 letters.	Placing one of 25 letters.	Finding one of 16 letters.	Placing one of 16 letters.	Finding one of 25 letters, with 9 letters absent.			Finding two of 25 letters.
					Average.	Absent.	Present.	
J. J.	$(^{20})$582	$(^{13})$309	$(^{10})$413	$(^{7})$316	$(^{10})$915	1085	817	$(^{7})$1445
W. S.	$(^{16})$485	$(^{11})$210	$(^{10})$302	$(^{10})$175	$(^{10})$649	722	610	———
L. C.	$(^{13})$640	$(^{10})$355	$(^{10})$428	$(^{10})$288	$(^{10})$761	1048	651	$(^{12})$1836
Aver.	569	291	381	260	775	952	693	1640

The numbers in parentheses indicate the number of sets of 20 observations from which each average was derived; the

[1] The essential features of this apparatus as well as of the problem investigated were suggested by Prof. G. Stanley Hall and were elaborated in conjunction with him at Johns Hopkins University some years ago.

other numbers represent the average times in σ=.001 second. We see that it took on the average 569σ to find one of 25 letters and 381σ to find one of 16 letters. The process is thus quite complicated and is very difficult at first, the stage of initial practice being quite marked and the first few sets yielding very long times. Considerable of this time is consumed in the process of accommodating the eyes to the plane of the letters and bringing them clearly into view. We considered that this time would be measured by measuring the time needed to see what letter occupied a certain position amongst the twenty-five. Instead of calling a letter and reacting when its position was seen, a position was called, for example A1, C3, etc., and the subject reacted when the letter occupying that position was recognized. The subject here knows just where to look and, although this time includes the recognition of the letter as well, we should remember that it is probably fair to exclude this element from the time of finding letters, the finding time strictly applying only to the process of search. While therefore only an approximate elimination of the mechanical process is obtained by subtracting the "placing time" (as we shall call this latter step) from the "finding-time," yet this difference very fairly represents the distinctive part of the finding process and is remarkably alike in the three subjects, 273σ, 275σ and 285σ.

The effect of the number of objects amongst which one is to be sought, and of the larger field is illustrated in the difference of time between finding one out of 16 and one out of 25 letters; this is on the average 188σ and in the individuals 169σ, 183σ and 212σ. The ratio of the times to find one out of 25 and one out of 16 letters thus increases in the proportion of 1.55 to 1 which is just the ratio of 25 to 16.

In "placing" a letter, that is, in recognizing what letter occupies a certain position, it is obvious that the time should be little, if at all, affected by the number of places, and the slight difference between the values found for placing one of 16 or one of 25 letters, 260σ, and 291σ is probably due to the fact that the former sets represent a more advanced stage of practice than the latter.

The next variation presents an interesting difference; 16 letters are present and, as before, these change with every observation, but instead of calling only for those letters that are present, any one of the 25 letters may be called for, and if not present the subject reacts as soon as he is convinced of its absence. The average result of all the experiments performed in this manner is 775σ; this, however, is not as significant as the result we obtain by considering separately those cases in which the letter to be found was present and those

cases in which it was absent. That it should take longer to go through a series of 16 letters and determine that a certain one is absent, than to determine its presence, is to be expected; the difference is certainly great whether we compare it with the finding time of one of 25 letters or more properly with the finding time of one of 16 letters. It takes 571σ longer, or 2½ times as long, to determine that a given letter is not among a group of 16 than to find it if it is present. But while it takes 381σ to find one of 16 letters when the subject knows it is there, it takes 693σ to perform precisely the same process when there is a chance (strictly when there are 36 chances in a 100) that the letter he is seeking may be absent. This result most strikingly illustrates the effect of the fore-knowledge of the subject upon the time of mental processes; the apparently simple process of comparing an objective with a subjective image varies its character according to the underlying connection by which the process is accompanied. This result, too, appears in the fact that, while in finding letters all of which are known to be present, an error is exceedingly rare, when the letters may be absent. Errors are quite numerous and consist in declaring a letter that is present to be absent.

In certain processes it is relatively easier and quicker to do two things together than to perform them separately; this being due to an overlapping of the mental processes. There is a division of the attention among the several mental tasks so that the time needed for the whole is considerably less than the sum of the times needed to do each separately. In other cases the attempt to perform processes together seems to result in a mutual inhibition or confusion and a loss of time and energy. As a small contribution to an investigation of this problem we determined in two subjects how long it takes to find two letters among 25 and to note their positions. The two letters were announced beforehand and as soon as both were found the subject reacted. This proved to be a very difficult and often confusing process; it took on the average 1640σ which (for the two persons under consideration) is 418σ longer than twice the time needed to find one letter. This may serve as an index of the loss of energy in attempting to have two processes before the mind simultaneously.

While our results are not sufficiently numerous or free from great variation to warrant detailed inferences, yet there are two such questions of detail the importance of which justifies even the mention of the imperfect information we are able to give. The first relates to the difference in ease in recognizing the various letters. That such differences occur has been shown by more suitable methods. Our results show considerable variation; for one subject the range is from

393σ for W to 557σ for T; for another from 487σ for S to 719σ for L. On the whole the three letters most quickly found were S, O, and W; and the four least quickly found L, J, H and T. If we ventured to divide the alphabet into three groups of easy, medium and difficult letters, our lists would read: 1. S, O, W, N, D, C, E, I; 2. X, B, Z, G, M, Y, A, R, B; 3. K, U, F, V, T, H, J, L. It must be remembered however that no great weight is to be placed in this detailed result. The second question involves the query whether the letters nearer the centre of the block are more readily found than those away from the centre. Our results are unfortunately not recorded in such form as to readily allow of the determination of this point; but we compared the times for all the letters found in positions B3, D3, C2 and C4, that is, in a diamond about the central letter C3, with those for finding the four positions furthest removed from the centre, A1, A5, E1, E5. Our result showed a slight excess of time for finding the peripheral letters, an excess too slight perhaps to be recorded were it not for its constancy in all three individuals.

This first attempt to gain a deeper insight into the mental process of finding certainly leaves untouched the larger number of important and suggestive queries attached to it, and yet the results obtained are sufficiently clear and consistent to justify the promise of future investigation.

Some Anthropometric and Psychologic Tests on College Students.—A Preliminary Survey.

(With the assistance of George W. Morehouse, Fellow in Psychology.)

During the fall of 1890 it was decided to ask the students in the general class in Psychology to lend themselves to series of physical and psychological tests with a view of interesting the students in such tests as well as acquiring a body of statistical material which when sufficiently extended and properly compared with other statistics might prove of considerable value.

The experiments were not extensive in character but they served to bring out the difficulties in this line of work, and the publication of the present fragmentary results[1] is ventured in the hopes of furthering similar observations elsewhere.

The tables given below require more or less explanation and comment. The physical measurements of the men are in the

[1] Simple and few as the tests were they required about 50 minutes for each student. If the tests could be arranged so that several persons might be tested together without interference a great saving of time would result.

main those regarded as most important by Mr. Galton, and were made with the intention of correlating mental with physical characteristics. The apparatus employed was very simple and hardly needs description. The dynamometer is of the Feré pattern, made by Cullin, Paris. Similar measurements for the women were obtained through the courtesy of Miss Ballard, in charge of the Ladies' Gymnasium, but were too few in number to warrant tabulation.

In four cases the measurements made by Mr. Galton upon miscellaneous Englishmen are exactly repeated upon these college students, and the results indicate in so far as such few results can indicate, a superiority in favor of the college students.

The sensibility tests were selected to quickly yield a few typical results. Like all such observations the chief difficulty lies in the fact that the subjects are not used to accurately observing their sensations, so that a relatively brief practice would in many cases alter the result. The æsthesiometer employed was that described in this JOURNAL (Vol. I, p. 552). It appears that the distance at which two points could be felt as two on the back of the hand was 16.4 mm. and on the finger-tip 1.63 mm.; the former result being strikingly small as compared with Weber's tables.

The sensitiveness of the palm was tested by determining the minimum height from which the fall of a bit of card-board could be perceived. These bits of card-board weighed .9 mgr. and were cut in rectangles of 1 by 2 mm. from a sheet of millimeter paper pasted upon the card-board.

The apparatus used for testing the pressure sense was a modification of Fairbank's post-office balance in which the weights were placed upon the scale pan, thus exerting an upward pressure upon the finger resting upon a cushioned plate at the end of the beam. A comfortable and firm position was secured and an attachment provided by which fatigue was prevented. Two-sevenths of the weight on the scale pan acted upon the finger. The table records that additional weight (to the nearest 25 gr.) which could be correctly distinguished about 3 or 4 times from an initial weight of 500 gr. But few observations were taken and the result is only approximate. The general result is that a difference of about $\frac{1}{6}$ or $\frac{1}{7}$ of the initial weight may be correctly appreciated.

We also attempted to measure sensitiveness to pain. For this purpose we used a light hammer (weight 98.3 gr.) pivoted at a point 200 mm. from the center of its iron head, and allowed it to fall on the tip of the fore-finger of each hand. The back of the hand as well as the finger struck was supported. The table records the minimum number of degrees

through which the hammer must fall in order to cause a painful sensation. While this is naturally not a clearly defined point, still its constancy was surprising. The left hand appears to be more sensitive than the right. As few falls of the hammer as possible should be used in this test as the skin rapidly fatigues.

We take up next a description of the tests of vision. The printed page was first placed beyond the subject's vision, then gradually moved toward him along a sliding scale until he could just read it. The column of the table gives the distance at which, with the maximum strain, the page could be read. The size of the type is that in which this article is printed. The same page was then held as close to the eye as possible and yet have the subjet able to read it. We next record the smallest size of print (in dioptrics) that could be read at 25 feet.

For the next test we prepared a large white disc with small black sectors ranging from 1° to 15° and proceeding by half-degrees up to 5°. When this was rotated there appeared a series of concentric rings of various light shades of gray, each ring being 10 mm. wide and separated by 5 mm. from its neighbors. The subject counted as many concentric rings as he could see, and the result was then read off in degrees.

The acuteness of vision was tested in several ways, (A), by finding the distance at which a series of black lines 1 mm. wide and separated by spaces of 1 mm. could be recognized and the spaces between the lines clearly discerned, (B), by a similar determination with a checkerboard pattern, both black and white squares, being 4 mm. square, and (C) by the distance at which either 7 or 8, 11 or 12 and 15 or 16 dots 2 mm. in diameter and irregularly arranged in a rectangle of 25 x 40 mm., could be counted. The results are recorded in inches.

Our next test related to color and we attempted at the same time to detect any color defects, and to get some measurement of the rapidity and accuracy of color distinctions. Each student was required to match as rapidly as possible 30 colored ovals of a Magnus-Jeffries Color Chart (as published by Prang). We also noted irregularities in matching. The average time shows about six seconds for each color.

The strength of vision we tested by noting the smallest size of letter readable at 25 ft. through one and through two thicknesses of common cheese-cloth. No student could see the letters at all, up to 50 dioptrics through three thicknesses. The result is recorded in dioptrics.[1]

[1] The only test for hearing that we attempted was to determine from what height a shot weighing 10 mgmm. must be dropped upon a glass plate to have the sound heard by the subject at a distance of 25 ft. The

We also made a few tests of the rapidity of movement. This was done by arranging two keys so that the closure of the one would start a Hipp Chronoscope and of the other would stop it. The distance between the keys was in the one case 38 inches and in the other case 3 inches. With the keys 38 inches apart the subject was first told to touch them in succession, not as fast as possible but at any rate which seemed natural to him. He next made a movement of the same extent, as well as one of 3 inches, as fast as possible. This was done separately for the right and left hands, and the average time of about 5 movements is recorded in the table. The movement must be somewhat accurate in order that the key shall be struck at each end. The results for the maximum movements enables us to determine that the movement alone was at the rate of about 8 feet per second.

It had been our intention to meet each student a second time and with this intention we inaugurated a series of tests of sense-judgment, only a very small portion of which was completed, namely those relating to pressure and one relating to the space sense of the skin. The subject was first required to pour as much shot in the palm of his right hand as he thought would weight an ounce. The average weight of the shot thus estimated to weigh an ounce was 37 gm., or an exaggeration of 13% (men 47 gm., an exaggeration 65%; women 22 gm., an underestimation of 21%). He was next asked to pour as much shot into a box ($3\frac{1}{4}$ x $3\frac{1}{4}$ x 4 in. made of $\frac{1}{8}$ in. pine) as he thought necessary to have shot and box weigh one ounce. In this case the average result was 97 gm. or an exaggeration of 242% (men 100 gm., exaggeration 252%; women 92.5 gm., exaggeration 226%). The illusion involved in this test is the well known fact that a stimulus spread over a larger area seems much less intense than a like stimulus confined to a more limited area. The result, in the two cases given above, measures the degree of the illusion. He next repeated the operation with the intention of making the box and shot weigh one pound. The average result was 548 gm. an exaggeration of 28% (men 605 gm., an exaggeration of 34%; women 463, an exaggeration of 2%). We find here a smaller percentage of exaggeration than in case of the ounce. He was then given the box which he regarded as one pound and irrespective of its actual weight was asked to put enough shot into another box to make it

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average result 27.8 mm. is inaccurate owing to the impossibily of securing absolute and constant quiet. It is interesting to note that the hearing of the women was more acute than that of the men, the results being 17 and 35 mm. respectively.

weigh double the first. The average result was 879 gr. or an underestimation of 20% (men 940 gr., underestimation 23%; women 789 gr., underestimation 15%).

The space-test consisted simply in spreading the points of the æsthesiometer on the back of the subject's hand until he regarded the distance between the points to be one inch. The average result was 30.6 mm., an exaggeration of 20% (men 31 mm., exaggeration 22%; women 30 mm., exaggeration 18%).

It is interesting to note that in all these tests of sense-judgment the women are more correct than the men.

In addition to this a few tests on bilateral symmetry of motion were made upon 17 of the lady students. They were asked to move the fore-fingers of the two hands outward from a common point along horizontal bars of a wooden cross the intention being to move the two arms to an equal distance. The movements were first made with the fingers at all points resting on the bar and were further subdivided into fast movements and slow movements, and again into large movements and small movements. All these variations were also gone through with for movements in which the fingers were lifted up into the air and brought down upon the bar at the end of the movement, (free movements). The table shows the result from each of these variations. It appears that, in each case, the right hand makes the larger movement, the excess on the average amounting to 15.5 mm. Regarding the extent of the excess of the *preferred* hand it is necessary to note that one student is markedly left-handed and another nearly ambidextrous. In both these cases the left hand makes the larger excursions and thus the average excess of the *preferred* hand becomes 16.7 mm. or $\frac{2}{3}$ of an inch.

It appears that the most influential of the distinctions made is that between the guided and the free movements, the average excess of the preferred hand in the case of the guided movements being 10.1 mm. and in free movements 23.4 mm. The size of the movement is of some influence upon this excess, it being on the average 21.3 mm. for the large movements and 12.1 mm. for small movements. In slow movement the excess of the preferred hand is more marked than in fast movements, being 19.9 mm. in the former and 13.5 in the latter. Individuals show considerable difference in the amount of this excess of the preferred hand, the average excess for the 17 different individuals being as follows: 54.3, 30.7, 30.1, 25.1, 22.6, 20.6, 17.6, 17.0, 12.8, 12.6, 10.9, 10.7 (left), 9.9. 9.0, 8.0, 8.0 (left), and 3.9 mm.

In addition to the measurements given above we placed before them a series of miscellaneous questions in regard to

personal and family characteristics. From the answers to these questions we collect the following data: the average age was 21 yrs. 11 mo. (31 men 22 yrs. 4 mo.; 22 women 21 yrs. 4 mo.) Of the 53 students 45 were born in Wisconsin, 7 in adjoining states while 1 is of foreign birth. Regarding the birth-place of the parents, in 29 cases it is in foreign lands, 23 in New England States, Vermont predominating, 32 in Middle States (N. Y. 28, Penn. 4), 21 from Western States.

The occupation of the father was noted with the following result: 15 merchants and manufacturers, 10 farmers, 13 professional men, 5 officials, 4 mechanics, 5 bankers and real-estate dealers.

When asked to state whether they regarded their health as "excellent," "good," "middling" or "poor," 20 (14 men and 6 women) pronounced it "excellent," 28 (13 men and 15 women) "good," 4 (3 men and 1 women) "middling" and 1 "poor." When questioned as to the existence of headaches or other chronic complaints 30 (16 men and 14 women) declared themselves free from all such, 13 (9 men and 4 women) were troubled with headache and 7 with other complaints.

46 of 52 students (27 men and 19 women) called their sleep "regular" and the rest "irregular," and 33 of 46 students (23 men and 10 women) spoke of their sleep as "sound," and the rest as "light." The average duration of sleep was just 8 hours.

It will be interesting to compare, as far as possible, the records of the men with those of the women. The general result regarding dermal sensations is that women have finer sensibility than men. This is true for each one of the tests made, but the differences are comparatively slight, except for the absolute sensitiveness of the palm and the sensitiveness to pain. The greater sensitiveness in women in both of these cases indicates freedom from rough usage.

As regards vision the differences on the whole are so small as to prove no superiority in the one case or in the other. To this there is but one exception and that is in the accuracy and rapidity of color perception in which the women are clearly better than the men.

Finally regarding the rate of movement, the normal movements, that is those adopted when no special direction is given, are quicker in women than men while the maximum movements, particularly in case of the longer movements, are faster in men. All these differences are consistently related to well recognized differences in the two sexes regarding the use and development of the different senses.

Table I.

Physical Measurements (of 31 Men, in mm.)

Height Standing.[1]	Height Sitting, from Seat of Chair.	Span of Arms.	Chest Girth.	Head Girth.	Strength of Squeeze.[2]
1748	926	1813	910	575	41.25

Table II.

Sensation and Movement.

Dermal Sensations.

Two Points Felt as Two.						Sensitiveness of Palm.			Pressure Sense.			Sensitiveness to Pain.					
Back of Hand.			Tip of Forefinger.									Right Hand.			Left Hand.		
T.[3]	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
16.4	17.5	15.0	1.63	1.71	1.52	44.0	58.2	21.9	82.5	83.7	80.7	26.7	33.9	16.6	19.3	22.7	14.8
(52)[4]	(30)	(22)	(54)	(32)	(22)	(49)	(27)	(22)	(53)	(31)	(22)	(53)	(31)	(22)	(52)	(30)	(22)

Sight (53 Students; 31 men, 22 women).

Distance at which print can be read.			Near point for print.			Smallest type visible at 25 ft.			Differentiation of white from gray.			Time for sorting 30 colors.			Distance at which lines can be recognized.		
T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
52.9	53.5	52.1	2.5	2.4	2 7	8.3	9.4	6.7	2.62°	2.74°	2.42°	177″	212″	130″	108	117	97

Sight, continued, (53 students; 31 men, 22 women).

Distance at which dots can be counted.									Distance at which checker-board pattern can be recognized.			Letter visible through cloth.					
7 or 8.			11 or 12.			15 or 16.						1 thickness.			2 thicknesses.		
T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
157	155	159	141	141	140	101	108	91	122	121	124	22.0	24.7	19.0	43.5	45 0	42.0

[1] The height of heel (average 21.2 mm.) has been subtracted from full height.
[2] This measurement was taken upon only 16 men and is expressed in kilograms.
[3] M is the result for the men, W that for women, T the average of both.
[4] The figures in parentheses give the number of persons tested.

Rate of Movement (45 students; 28 men, 17 women).

Movement through 38 inches.												Movement through 3 in.					
Normal.						Maximum.						Maximum.					
Right Hand.			Left Hand.			Right Hand.			Left Hand.			Right Hand.			Left Hand.		
T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
1000[1]	1070	885	908	964	817	542	506	601	527	498	574	181	181	181	185	172	205

[1] These numbers indicate $\sigma = .001$ sec.

TABLE III.

Symmetry Movements.

Guided.								Free.							
Fast.				Slow.				Fast.				Slow.			
Large.		Small.		Large.		Small.		Large.		Small.		Large.		Small.	
R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.
496	494	179	170	493	505	190	177	505	497	199	185	510	480	180	168

Addition to Literature Notices under article on Zöllner's Illusion.

MÜLLER-LYER (Du Bois Reymond's Archiv. Supp. Band 1889) gives a brief but valuable account of a variety of optical illusions of judgment. He clearly demonstrates the influence of angles, of positions of figures, and the like upon their apparent size. His explanation of the illusions refers them to the tendency of considering surrounding and suggested areas in the judgment of lines and areas. He also mentions the effect of the angle in Zöllner's illusion, but does not enlarge upon its relation to the other illusions. The article, while comprehensive and original, does not add materially to the explanation of the illusion[1].

[1]The illusions of contrast in our article are described in Müller-Lyer's article. While I had read this article in 1889, I had entirely forgotten about it in the present investigation and worked out the present figures, which I had not seen before (they are not figured in Müller-Lyer's article but only incidentally described) independently. Dr. Sanford has drawn the figures described by Müller-Lyer, and through him my attention was again called to this figure and article *after* the present article was written.—J. J.

Corrections to "Studies from the Laboratory of Experimental Psychology of the University of Wisconsin." AM. JOURNAL OF PSYCHOLOGY. Vol. IV., No. 2.

On page 199 insert the following table, accidentally omitted:

RANGE OF WORDS.	J. J. σ.	F. W. σ.	MOTOR. J. J.	MOTOR. F. W.	SENSORY. J. J.	SENSORY. F. W.
Any word whatever	269	267	266	262	272	272
One of 100 verbs	260	265	253	263	265	267
One of 50 animals	250	262	250	256	250	268
One of 20 names	238	249	233	246	243	252
One of 20 letters	238	243	237	233	239	252
One of 10 French words . . .	245	251	246	249	244	253
One of 10 numbers	229	233	227	232	231	234
Simple Reaction Time	177	187	174	184	181	191

The pages in "Accessory Apparatus for Accurate Time-Measurements" belong to the study of "The Effect of Foreknowledge upon Repetition-Times," and the "Note upon Apparatus and Method" (p. 200) is a part of the former.

The "Note A—On the Timing of Rotating Discs," and the "Note on a device for color mixing" (p. 211) belong to the study of "A Novel Optical Illusion," and should be credited to Mr. Moorehouse. In the cut (p. 210) the letters B P on the right hand side should be B′ P′.

SERIES FOURTH.

FROM THE AMERICAN JOURNAL OF PSYCHOLOGY.

Vol. 5. No. 2. Nov. 1892.

Studies from the Laboratory of Experimental Psychology of the University of Wisconsin.

ON THE JUDGMENT OF ANGLES AND POSITIONS OF LINES.

A.—On the Judgment of Angles.

By Joseph Jastrow, Ph. D.

With the assistance of Geo. W. Moorehouse, Fellow in Psychology.

The nature and extent of our errors in estimating and reproducing angles are the subject of our present inquiry. The point acquires a special interest from the fact that a number of writers have based their explanations of important optical illusions upon the view (apparently not tested by experiment) that acute angles are underestimated and obtuse angles overestimated. Such an investigation naturally begins with a definite mode of judgment or reproduction under definite circumstances, and is then to be supplemented by an investigation of the extent to which the results obtained are due to peculiarities of the method employed. We selected the angles 15°, 30°, 45°, 60°, 75°, 90°, 105°, 120°, 135°, 150°, 165° as our standard angles and drew these upon circular pieces of cardboard (3 in. in diameter); the lines themselves were 30 mm. long and were so placed that one line was always horizontal. The papers on which the angles were to be reproduced by the subject were placed on a table and the drawings made in the normal writing position. For convenience all acute angles were formed on the left hand side and obtuse ones on the right, so that the drawing might be uniformly from left to right. The subject viewed the angle as long as was needed to fix it in his mind (from 5 to 15 seconds) and immediately thereupon, from his memory of the angle, drew another as nearly as possible equal to the first. He did this by adding a line to a horizontal line of 30 mm. which was given him (always in the same position) upon the squares of paper upon which he drew. The drawing was done with a hard, well-pointed lead-pencil. Each of the standard angles occurred twice in a set of 22 angles, the order of the angles being determined by chance. The results of the measurements of 62 such sets, or 124 reproductions of each angle by 13 subjects in all, are shown in the following table:

Standard Angle	15°	30°	45°	60°	75°	90°	105°	120°	135°	150°	165°
Average Reproduction	18°03′	30°27′	42°09′	56°25′	69°44′	90°06′	113°34′	123°55′	136°29′	150°33′	163°49′
Error (from corrected standard)[1]	+2°23′	-0°56′	-2°16′	-5°13′	-6°46′	+0°06′	+7°54′	+3°22′	+0°19′	-1°05′	-2°41′

[1] For details of measurement and preparation of tables and curves, consult the note at the end.

The great value of watch-dogs depends chiefly on two faculties, their acute sense of hearing and the restlessness they feel when they hear the least strange sound. By properly directing these faculties a good watch-dog is produced. In the training of dogs habits are to be acquired through a repetition of experience. Mental and physical qualities must be developed together. The natural instincts of the dog form the basis of training. Commands are given by voice and hand and the dog is accustomed to associate certain actions with certain signs, obedience with reward, and desobedience with punishment. The trainer should often himself do what he wishes the dog to perform. Thus when a shepherd trains his dog to lie down at the word "down," he lies down himself and makes the dog do the same.

Patience and time are always necessary, but the dog's natural instinct and intelligence aid in the speedy formation of habits, and with succeeding generations, owing to the power of heredity, the work is less difficult.

The Dog in Health and Disease, WESLEY MILLS. New York, 1892, pp. 407.

A complete and valuable work chiefly in regard to canine pathology, but also containing a chapter on breeding and training. Dr. Mills says: "A puppy, full of life, tends to do exactly as his impulses urge him, till the highest motive power, a desire to please his master, is substituted." Training, therefore, is a question of the direction of impulses by the formation of pleasant and painful associations, leading finally to the establishment of the habits desired. With regard to the methods to be employed the author is in accord with the best authorities.

It will be seen that the overestimations and underestimations can hardly be said to follow any simply formulatable law, such as the underestimation of acute and the exaggeration of obtuse angles; their full significance appears only in the curve as given below (Fig. 1); in this curve the differences, between the actual and the re-

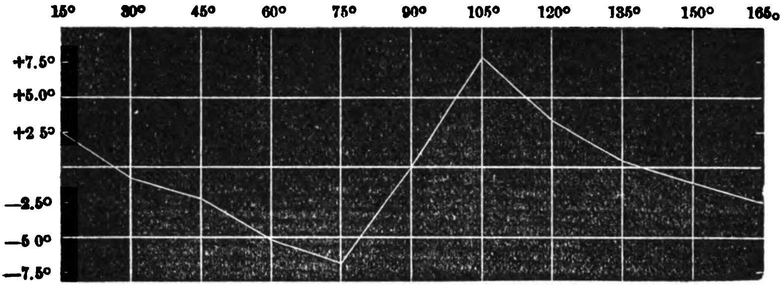

Fig. 1. Errors, in degrees, in reproducing angles by memory: Average of 13 subjects.

produced angles are plotted to the scale of one of the divisions to five degrees of error. This is our general result and its salient characteristics are, (*a*) the exaggeration of the angle at 15°, which passes into a gradually increasing underestimation up to 75°; (*b*) the correct reproduction of right angles; (*c*) the maximum exaggeration of the angle of 105°, which is followed by a decreased exaggeration, passing into an underestimation of very obtuse angles.

The next important inquiry is naturally how far the result is typical, and how far accidental; how far the result of the combination of different curves and how far the individual records agree with the general result.

By each of the thirteen individuals the angle of 15° is exaggerated; in all of the thirteen cases there is a falling off towards the next point 30°, the angle being about as frequently slightly overestimated as slightly underestimated; in all but two cases there is a fairly regular increase of the underestimation, reaching a maximum at 75°; in all cases the right angle is nearly correctly reproduced, the error being as often in one direction as in the opposite; in all cases the curve then sharply rises, reaching the maximum of exaggeration at 105°, and from there in eleven cases there is a more or less regular decline; the curve at the last point 165° falling below the line. Again, we may calculate the average deviation of the thirteen results from their mean; this for the eleven angles is 2° 31′, and of the 143 records (13 subjects for 11 angles) 87 show a deviation less than this average. Regarding the variation for the different angles it is least for 90°, the other angles following in this order, 150°, 165°, 15°, 30°, 135°, 75°, 45°, 120°, 105°, 60°.

Comparing the general outlines of the individual curves with the average curve, we find that in ten of the thirteen individuals, the correspondence is obvious and in most of these, striking; in one case the curve presents quite a different appearance, and in two other cases the differences are considerable. This diverging curve, however, is that of a professor of engineering, who has considerable experience in the estimation of angles; he draws angles of 30°, 90°, 135° and 165° very accurately, overestimates angles of 15°, 60°, 105° and 150° considerably, and underestimates the angles of 45°, 75° and

120°, thus presenting a zig-zag curve. The other two subjects came prepared with some practice in drawing and exhibited peculiarities in the mode of estimating the angles. In general there is thus a very striking similarity between the individual and the general result, so that the curve may be regarded as fairly typical for the average person.

The suggestion is close at hand that this result may be influenced by the mode of reproduction; to test this, three of the subjects reproduced the angles, not from memory but with the standard angle constantly visible for comparison. The resulting curve is quite similar in the two cases, the essential difference being that the entire curve is closer to the true line of no error, i. e., the error is smaller. Fig. 2 shows the average result of the three subjects for each mode of reproduction.

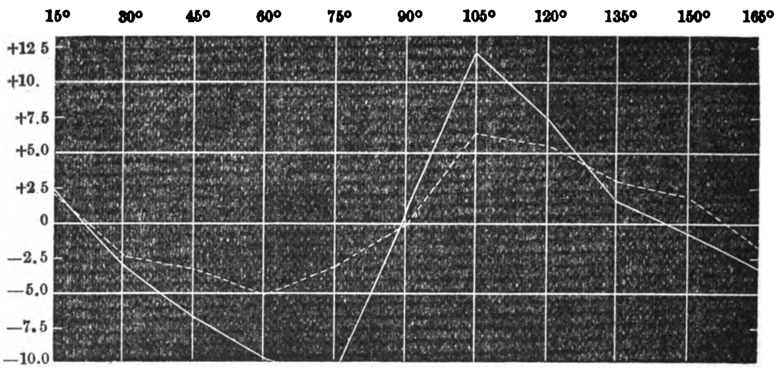

FIG. 2.

......Errors in reproducing angles with both angles visible. Average of three subjects.
———Errors in reproducing angles by memory. Average of same three subjects.

It is likewise interesting to determine the regularity and accuracy of these methods of reproducing angles; a sufficient test of this is the average deviation of the results from their mean. This is very troublesome to calculate and we have contented ourselves with doing it for two individuals. In reproducing angles by memory the average deviation of the one subject (fourteen judgments of eleven angles) was 2° 50′, of the other 3° 05′; in reproducing angles with the standard angle in sight 1° 33′ for the one and 2° 08′ for the other. There is thus indicated an increase of regularity with the decrease of absolute error. A comparison of deviations for the several angles shows that the right angle has by far the smallest deviation (about one-fourth of the average), and that the smallest and largest angles are somewhat more regularly reproduced than intermediate ones, and thus again indicate the direct relation between error and regularity.

Finally as to the significance of these results, we may offer the following suggestion. Our curve may be viewed as consisting of two portions, the first beginning with 15° and ending with 75°; the second beginning with 105° and ending with 165°, i. e., we omit the right angle as well as the angle 0°. In that case the curve falls into two (often strikingly) similar portions, beginning with an exaggeration and ending with an underestimation. This would mean that angles with a small excess over 0° or 90° are more exaggerated

or less underestimated than angles with a greater excess over 0° or 90°, and in this special sense is it true that acute angles are underestimated and obtuse angles overestimated; the smallest and largest angles forming an exception to the generalization.

More on Details of Method.—The angles were measured by applying a square of card-board 25 mm. square to the horizontal line, having the apex of the angle coincide with a corner of the square; the distance of the intersection of the oblique line with the side of the square was noted to the nearest ¼ mm. and from this the tangent of the angle could be readily calculated; for the above process gave us the measurement of the opposite side of an angle whose adjacent side was always 25 mm. In this way under favorable circumstances a set of twenty-two angles could be measured and the results tabulated in five minutes. There is inevitably some error in this mode of measuring, and to eliminate such error, as far as possible, we measured our standard angle by the same method, finding as a result the angles 15° 40′, 31° 23′, 44° 25′, 61° 38′, 76° 30′, 90°, 105° 40′, 120° 33′, 136° 10′, 151° 38′, 166° 30′; the deviations plotted in the curves are from these angles and not from the theoretical angles 15°, 30°, 45°, 60°, 75°, 90°, 105°, 120°, 135°, 150°, 165°. In comparing the general average, the average of each individual was weighted by the number of sets of which it was the average.

B.—On the Judgment of the Positions of Lines.

With the assistance of James H. Turner.

Several points in the results just described suggested further research. The fact that one side of each angle was given as well as that the lines are drawn on square pieces of paper with one line parallel to the side of the square, may have important influences upon the results. To eliminate entirely the influences which these conditions may have induced, it seemed necessary to ensure an environment for the subject in which no straight lines whatever should be visible except those judged, for the lines of the floor and walls are manifestly sufficient to give him his vertical and horizontal and thus a basis for estimating angles.

To secure these conditions we arranged the experiment so that the subject could see nothing but one or two white card-board discs, four inches in diameter, upon which was drawn a straight black line three inches long. The two discs, one above the other, were viewed against a large black disc thirty inches in diameter, all placed in a vertical position. Above his head was a parasol-like frame, from which hung black draperies and a similar black cloth was drawn across his lap. When in position he was completely enclosed under this canopy, the light coming in from the back above his head; no portion of the floor or walls was visible to him. He was seated on a chair with his eyes on a level with a point midway between the two discs and about 15-20 inches away from them. The subject's arms reached outside of this canopy and held two handles attached by cords to the axle upon which the upper of the small discs turned. By pulling the right or left cord the subject could thus bring the straight line drawn upon the card-board into any position. This line was three inches long and 1 mm. wide and its centre was the centre of the four-inch disc. At the other end of this apparatus, which was firmly mounted upon a table, sat the observer, who had before him two circles divided to half degrees; to each of the other ends of the axles, upon one end of which was glued a four-inch disc, was attached a "hand" ending in a fine needle point, which was so ad-

7

justed as to assume precisely the same position as the line upon the disc. This adjustment was constantly regulated by setting the line vertical (by a fine plumb line) and noting the deviation, if any, of the hand from 90°. Finally a third axle midway between the two that bore the four-inch discs and thus in the centre of the thirty inch disc, bore a five-inch black paper disc eccentrically mounted and covering at pleasure either the upper or the lower disc.

A twisted cord attached to the axle and also to a hinged lever, the cord drawn and kept tense by a weight, enabled the operator by a simple movement to conceal either the upper or lower disc. Both were never in view at once. With this apparatus our method of experimentation is very simple. The operator sets the line of the lower disc at any desired angle; he then uncovers this disc, allowing the subject to view it until a clear impression of the position of the line is obtained; he then instantly covers this disc, and the subject, by means of the strings, sets the line of the upper disc to correspond to the remembered position of the lower line. The operator reads the position on his divided circle and the difference in the two readings gives the error. In the meantime another position has been set on the (invisible) lower disc, which is now revealed, and so on. Eighteen positions were used, forming angles of 0°, 10°, 20°, 30°, 40°, 50°, 60°, 70°, 80°, 90°, 100°, 110°, 120°, 130°, 140°, 150°, 160°, 170° respectively with the horizon. The observations were taken in sets of eighteen, each angle occurring once in a set. With this apparatus we studied the error in setting, after a brief interval, one line in the same position as a standard line; this judgment clearly involves angles, for it is based partly at least upon the angles formed with ideal verticals and horizontals. It also involves the conception of parallelism, for the task may be conceived as that of setting one line parallel to another.

Ten individuals were tested, seven of them drawing each position of the line ten times, and three of them each line twenty times. The average settings in degrees and tenths of degrees of each of the ten subjects for each of the eighteen positions of lines are exhibited in the following table. The Roman numbers indicate the different subjects; the upper line, the standard positions of the lines. The average of all is shown in the lowest line:

	10°	20°	30°	40°	50°	60°	70°	80°	90°	100°	110°	120°	130°	140°	150°	160°	170°	180°
I.	9.6	23.5	31.7	37.8	49.0	60.8	69.7	78.7	91.2	104.2	113.7	122.5	131.7	140.9	153.7	162.0	171.5	178.5
II.	12.0	21.1	27.8	41.4	55.5	60.2	68.3	79.2	92.0	104.2	114.4	123.4	129.8	140.8	151.6	162.5	171.0	180.4
III.	10.2	19.9	29.1	41.9	52.0	61.4	74.1	83.8	91.2	103.6	111.9	124.8	131.9	146.6	155.9	167.2	176.2	181.4
IV.	9.6	18.4	28.5	40.5	47.9	55.9	63.9	71.8	90.6	105.2	111.9	121.1	131.2	144.9	153.7	162.5	174.4	180.7
V.	11.9	19.8	30.0	43.3	50.9	57.9	67.6	74.9	90.4	102.2	111.0	121.2	129.7	138.3	150.8	157.2	166.4	179.6
VI.	14.3	24.6	37.8	47.1	54.4	65.3	72.0	82.8	90.9	106.2	114.4	123.6	133.2	141.2	153.1	161.3	172.7	180.6
VII.	10.8	21.9	31.9	39.4	52.4	61.0	72.7	82.9	90.3	101.5	114.0	121.4	131.7	143.2	152.7	162.5	173.0	180.3
VIII.	9.5	24.3	31.6	37.8	49.8	60.6	70.0	82.7	90.8	99.2	110.2	117.1	134.2	141.4	148.7	162.9	174.5	181.7
IX.	9.2	21.1	33.5	44.2	53.6	62.5	73.0	81.0	89.6	100.7	114.8	123.4	132.5	141.8	155.6	161.2	170.3	180.2
X.	10.6	19.6	29.8	39.0	46.0	57.8	66.7	76.1	89.0	105.0	114.7	126.1	133.5	141.9	151.3	158.5	168.9	179.8
Average	10.77	21.42	31.7	41.24	51.4	60.34	69.80	79.39	90.6	103.2	113.1	122.46	131.94	142.1	152.91	161.78	171.89	180.32

If these averages be plotted in a curve similar to that drawn for reproduction of angles, a very irregular curve will result, presenting practically no constant characteristics. The errors vary with each angle and almost with each individual. All the angles are so nearly correctly reproduced that the order of their correctness seems almost accidental, although there are abundant indications that the vertical (90°) and the horizontal (180°) are more accurately reproduced than any others.

It is also true that the angles are rather more apt to be over-estimated than under-estimated, and that the obtuse angles are rather more over-estimated than the acute ones. Of the 180 records entered in the table, 138 are over-estimations and 42 under-estimations. Of the 90 records for angles of 90° and less, 55 are over-estimations and 35 under-estimations; of the 90 records for angles between 90° and 180°, 83 are overestimations and 7 underestimations. To this extent the characteristics of the former results reappear. As a very rough comparison of the errors in drawing angles from memory with one angle and the side of the other given, in doing this with both angles visible, and in judging positions of lines, it may be stated that the average error for all angles (without regard to their being positive or negative) of the three individuals whose records we have for all those methods are 3° 40′ in the first case, 2° 42′ in the second, and 1° 53′ in the third. The entire curve for positions of lines is thus nearer the line of no error than that for reproducing angles. It must be remembered that much of this resulting absence of error is due to the balancing of errors of opposite directions, particularly so with acute angles. The subject had, if anything, a smaller degree of confidence in the correctness of his reproduction of positions of lines than in those of angles, and had decidedly less confidence in the former than in the latter when these were drawn with both angles visible. An indication of the regularity of these reproductions is furnished by the average deviations of the individual reproductions from their mean. This for the three methods (average of all the eleven angles, eighteen positions) is for one individual 2° 50′ for angles from memory, 1° 33′ for angles with the standard angle visible and 3° 15′ for positions of lines; for another subject 3° 05′, 2° 08′ and 3° 49′. This would indicate the greatest variability, least regularity for the estimation of positions of lines, the least variability for reproduction of angles, with the standard angle visible, and an intermediate degree for reproductions of angles from memory. This order corresponds with the subjective feeling.

In general, then, we conclude that in the reproduction of positions of lines without reference to any but imagined coördinates, the absolute error is small; is on the whole an overestimation of the angle;[1] is greater with obtuse than with acute angles; while the individual variation of the results is rather large. No simply formulatable law is followed by the resulting curve of error; the errors varying irregularly with the angle and the individual.

B II.—On the Judgment of Horizontal, Vertical and Oblique Positions of Lines.

With the assistance of W. D. Brown.

It has already appeared that much of our perception of angles and positions of lines takes place by reference to an ideal vertical and horizontal which we constantly carry with us and have had

[1] This overestimation means that the upper end of the line was set too far to the right; this may be due to a greater dependence of the right eye in judging or in the adjustment of the right hand.

forcibly impressed upon us by the countless verticals and horizontals with which civilization has surrounded us. It would indeed be strange if this enormously extensive experience with right angles, verticals and horizontals should not have left its impress upon our psycho-physiological organism. We have had some evidence of it in the accuracy of judging right angles; and the importance of the subject led us to undertake the determination of the accuracy of this ideal vertical and horizontal. We did this with the apparatus above described, using only the upper disc. The subject simply set this disc until the line upon it appeared to him exactly vertical or horizontal. We also had him set it in a diagonal position 45°, with the vertical or horizontal, speaking of these as "left oblique" or "right oblique," according as the upper end of the line pointed to the left or right. Each set consisted of 20 settings, in which the four positions, vertical, horizontal, "left oblique" and "right oblique," occurred in a chance order.

Observations were made upon ten subjects, eight of whom set each line twenty-five times (five sets) and two of whom set each line fifty times (ten sets). In the following table appear for each subject the number of settings of each position, the resulting average for each position and the average variation of the twenty-five (respectively fifty) records from their mean value.

The last line of the table averages these results for the entire ten subjects:

Subject.	No.	Vertical.		Horizontal.		Right Oblique.		Left Oblique.	
	Obs.	Setting.	Variation.	Setting.	Variation.	Setting.	Variation.	Setting.	Variation.
J. J.	50	89° 26′	0° 31.7′	179° 29′	0° 34.4′	42° 02′	2° 29.4′	134° 58′	1° 52.5′
W. D. B.	50	89° 51′	0° 45.3′	179° 19′	0° 30.7′	44° 59′	2° 41.2′	135° 46′	2° 29.0′
F. S.	25	90° 40′	0° 49.2′	180° 16′	0° 35.8′	42° 49′	2° 52.2′	138° 41′	3° 12.1′
F. E. B.	25	90° 42′	0° 40.1′	180° 39′	0° 54.7′	36° 25′	4° 04.4′	141° 25′	3° 58.2′
E. P. S.	25	92° 35′	0° 34.2′	182° 14′	0° 34.1′	44° 08′	2° 36.7′	139° 44′	1° 22.3′
J. H. D.	25	90° 26′	0° 25.5′	179° 52′	0° 51.4′	41° 16′	2° 04.8′	139° 20′	2° 28.2′
J. H. T.	25	88° 53′	0° 36.9	178° 52′	0° 57.5′	38° 40′	2° 52.1′	137° 40′	2° 59.0′
C. M. R.	25	90° 02′	0° 31.2′	180° 13′	0° 36.2′	41° 17′	4° 07.3′	146° 12′	3° 37.2′
G. W. M.	25	90° 09′	0° 30.4′	180° 02′	0° 24.1′	42° 39′	3° 16.6′	136° 42′	4° 09.0′
E. T. V.	25	89° 40′	0° 33.8′	180° 13′	0° 31.4′	33° 41′	2° 08.9′	141° 29′	3° 16.1′
Average.		90° 14′	0° 35.8′	180° 07′	0° 39.0′	40° 50′	2° 55.4′	139° 12′	2° 55.4′

It appears at once that the ideal verticals and horizontals that we carry with us are exceedingly accurate. This is shown not alone by the close approximation to 90° and 180°, but by the very small average variation, less than two-thirds of a degree. The individual variation is also small; 40.8′ for 90° and 36.1′ for 180° amongst the ten subjects. The diagonal positions show a larger and more constant error; the right oblique, if exact, should be at 45°, but, in all cases, it is less than this. The left oblique, if exact, should but be at 135°, but in all cases [in the first line (J. J.) there is an underestimation of 02′] it is overestimated. This means that in both cases the oblique lines were placed too near the position of the horizontal, in the one case (right) by 4° 10′, in the other (left) by 4° 12′. The average variation is also larger than in verticals and horizontals, being nearly 3°. The individual variations for the ten subjects are 2° 42′ and 2° 26′.

No elaborate comment upon these results is necessary. They give evidence of how thoroughly we have been drilled in the perception of rectangular coördinates; and the small variation both of the individual records and of the subjects is especially noteworthy. Our perception of positions midway between the vertical and horizontal is not so accurate nor so constant, the tendency being to approximate them too closely to the horizontal. The error is the same in direction and extent as that for angles of 45° and 135°, when they were reproduced with the standard angles visible (see Fig. 2 above), but is out of relation to the corresponding reproduction of positions of lines.

A Further Study of Involuntary Movements.

With the assistance of Thomas P. Carter and Edward P. Sherry.

In a previous contribution (this Journal, Vol. IV. pp. 398-407) there was described an apparatus—the automatograph—by which involuntary movements in the direction of the attention could be readily recorded; and typical illustrations were given of such movements, obtained under various conditions. In further study of these movements we attempted to determine the effect of the position of the body upon them, to analyze them with their constituent factors, and to experiment upon certain other points closely related to these.

If the arm be extended to the side of the body, movements of the hand forward are more readily made than movements backward, and movements toward the body more readily than movements away from the body. The hand moves most easily along a circumference of which the shoulder is the centre. The desideratum is a position in which movements in all directions would be equally easy; while this is almost impossible to secure, it may be approximated by extending the hand at an angle of about 45° with the line joining the shoulders and with the elbows bent at an angle of about 120°. The hand thus extended is placed upon the centre of the automatograph—firmly fixed to the table—and a constant position is secured by outlining in chalk the position of the subject's feet upon the floor. In this way the differences in question were reduced, but not eliminated; the average of all the comparable records at our disposal shows half again as extensive a movement towards an object of attention to the front as towards one to the rear, and a third again as much movement towards as away from the body. In some cases, too, the tendency to move forward overbalances the tendency to move towards the object of attention; in such cases we should have, however, a smaller movement to the front than when the object of the attention was to the front. In brief, the difference in the records accompanying the direction of

the attention to the front and to the rear, seldom fails to appear, although it may appear as a difference in the amount of movement instead of in the direction of movement. Fig. 5 shows a case of the former kind, obtained under different but comparable conditions; Figs. 7 and 8 illustrate the more usual result. The fact that the movements recorded serve as an index of the direction of the attention may thus be established independently of the influence of the position of the body; a conclusion corroborated by results to be described presently.

Observation of the subject's movements during an experiment strongly indicated that the result was complex, and originated in several portions of the body; it seemed both general and local. The chief factor in the general movement was referred to a swaying of the body with the feet as a pivot; this swaying of the head we recorded by fixing the recording plate horizontally on the subject's head[1] and suspending above it the glass pencil, held in an adjustable arm, which was firmly fixed to an upright on the table. The device for holding the writing point is the same as that used in the automatograph and is shown in full size in Fig 3. A cork (C) is pierced by the snugly fitting glass tube (T) and within the tube the pointed glass rod (R) moves easily up and down, accommodating itself to all irregularities of surface or movement; a small rubber band (B) is useful in raising the pencil off the record and in preventing it from falling through the tube. For the record nothing is better than the small ground glass drawing-frames that children use. These nicely hold and stretch the glazed paper; they may be stacked without injury to the record, and the frame prevents the pencil from leaving the record. This device may be variously used and may be recommended as the simplest method of recording movements of the kind in question. It is to be noted that when the subject holds the record-plate and the pencil is fixed, there is recorded a movement in opposite direction to that really made.

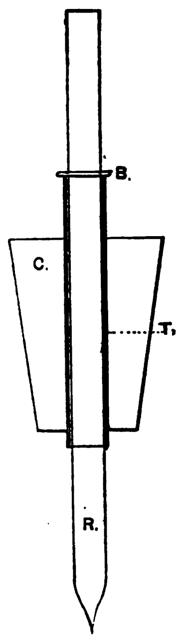

Fig 3. Device for recording movements. The glass rod (R) moves freely up and down in the glass tube (T) held in the cork (C). The rubber band (B) prevents the rod from falling throught the tube.

The movements of the head[2] show the influence of the direction of the attention similarly to those of the hand; indeed the correspondence between the two is considerable and often striking. It appears best when movements of the head and of the hand are recorded at the same time. Fig. 4 may serve to indicate the degree and nature of the correspondence; the head movements are apt to be more extensive and distinctive than those of the hand. This favors the conclusion—to be reinforced by other considerations—that the swaying of the body contributes an important part to the automatograph records.

[1] To fasten this upon the subject's head, a screw eye is fastened to each end of the frame holding the smoked paper; a rubber band is drawn over each arm up to the shoulder; to this band is affixed another rubber band passing through the screw eye and thus securing the frame upon the head. Some soft padding under the frame is desirable.

[2] The records for this study (Figs. 2-12 and 16) have all been taken upon the same individual and thus are as comparable as possible.

We failed to discover any constant tendency to sway in a special direction; movements backward seemed to be as readily made as those forward, and to the right as readily as to the left. When the attention is not directed in any special direction or is directed to a point overhead, an irregular forward and backward as well as lateral swaying results, which is quite different from that accompanying a definite direction of the attention.

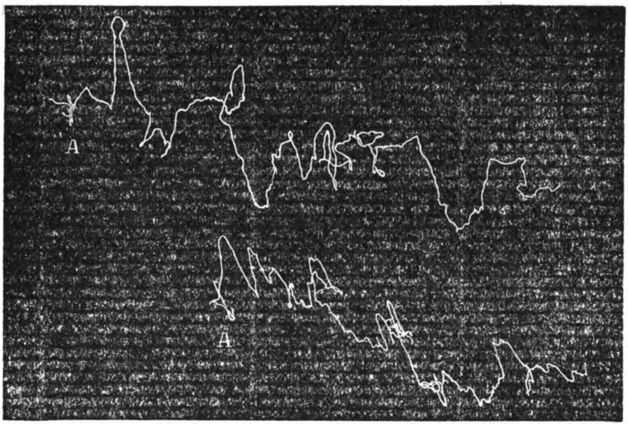

Fig. 4. Counting metronome. ⟶ Upper line, movements of head; lower line, of hand on automatograph; time, 45 seconds. The head movements are reversed, but have been again reversed for readier comparison. Figs. 4 to 13 are all obtained upon the same subject. The arrows indicate the direction in which the object attended to was situated.

The most obvious method of eliminating these swaying movements is to experiment with the subject in a sitting position. A typical record of the hand movement on the automatograph with the attention directed to the front appears in I, Fig. 5. The irregu-

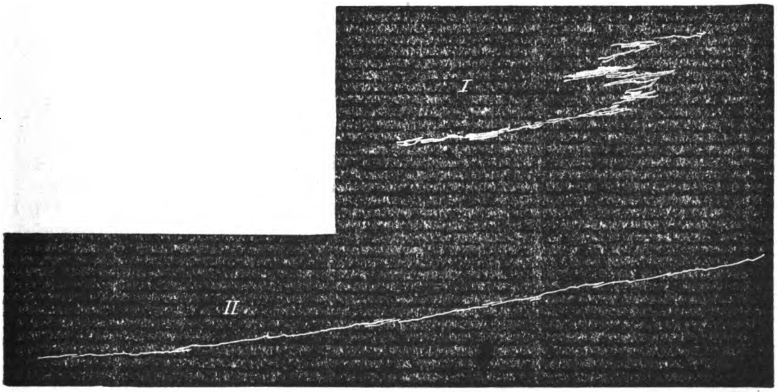

Fig. 5. Counting metronome. Facing ⟶. Automatograph, sitting. I, ⟵; time, 105 seconds. II, ⟶; time, 45 seconds.

lar lateral oscillations have nearly disappeared; the tendency to move along a circumference of which the shoulder is the centre is marked. A more satisfactory method of eliminating the swayings of the body consists of holding the pencil in one hand and the record-plate in the other; in this way pencil and record sway together and thus no record of it is made. Under these conditions we obtain a characteristic type of movement; the several oscillations are small and fine, as appear best when examined with a magnifying glass. Fig. 6 illustrates the type of movement very

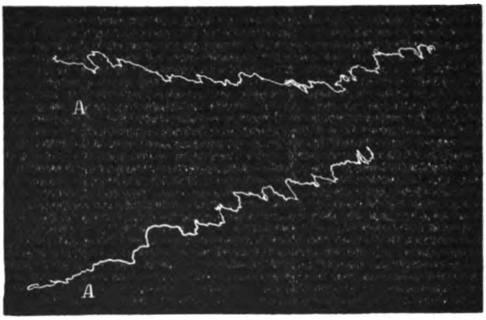

Fig. 6. ⟶ Counting metronome. Right hand holds pencil, left hand holds record; time of each, 90 seconds. Facing ⟶. Upper line, standing; lower line, sitting.

well. It further illustrates that by this method there is no difference, or but a slight one, between the records taken while the subject is standing and while sitting; which is precisely what should be the case if the general movements of the body have been eliminated. In this figure traces are observed of a somewhat regular periodic "curve"; these mark the respirations, and in II, Fig. 7, they are sufficiently distinct and regular to be counted, about twenty to the minute. It is natural that the respiration should appear, because the arm holding the record-plate is rested against the body, and thus records the abdominal movements, though these are apt to be obscured by involuntary movements of the hand holding the pencil. The tendency to move towards the object of attention appears throughout; Fig. 7 further illustrates a movement to the rear as well as to the front, and in Fig. 8 we have an unusually clear indication of readiness with which the direction of the attention may be received. The subject attends to and counts the beats of a metronome, which is in turn carried from one corner of the room to the next; the hand accurately follows the attention, yielding an almost perfect square. In all these tests it is important that a position be chosen in which movements in all directions are equally possible.

Fig. 7. Thinking of a building. Facing ↑; standing. Right hand holds pencil, left hand holds record; time of each 60 seconds, I, ↑; II, ↓; shows respiration.

As a further test of our analysis of these movements we recorded the movements of the two hands at the same time. This may be done by holding a pencil in each hand over a record-plate placed upon a table, or by holding a record-plate in each hand under a writing point projecting from a table or upright. The difference between these methods is not great; the former method allows of slight finger movements, while the latter does not. The latter is, on the whole, more convenient, because the natural sinking of the hand cannot spoil the record, which might be the case in the other method. The record-plate was placed upon a light board, to which a handle set vertically or horizontally could be attached. Both methods admit of a variety of positions of the arms and hands and dispense with the necessity of maintaining the record-plate level[1]. The results show that the movements of the two hands are very similar indeed; part but not all of this similarity is due to the swaying of the body, which would naturally affect the two sides alike; but there seems also a tendency for the two hands to move together in following the direction of the attention. Fig. 9 illustrates the close similarity of the movements of the two hands. It is important to remember that the records must be made in the same way and the hands be held in the same position. The tendency to move is greater when the hand is held away from than when held close to the body; Fig. 10 illustrates this difference and at the same time shows the correspondence of the form of the movement, notwithstanding the difference in extent.

Fig. 8. Counting metronome. Right hand holds pencil, left hand holds record. From A to B, ↑; from B to C, ⟶; from C to D, ↓; ↑ from D to E, ⟵; standing. Each part, 45 seconds.

We have thus illustrated a variety of methods of recording involuntary movements and of analyzing the chief factors contributing to the result. In a measure this separates the mechanical from the psychological portions of the movements and sheds some light upon the positions and methods used in muscle-reading; the additional facilities derivable from the movements of locomotion should not be overlooked.

To this account may be added a few illustrations interesting from various points of view. Involuntary movements are naturally not limited to the horizontal plane; the rod sliding within the tube simply records these alone. We may fix the record-plate in a vertical position against the wall and take the cork between the fingers of the outstretched hand, holding the tube in a slanting position, and thus record vertical movements. This is, however, a fatiguing position, and the fatigue is manifested in a sinking of the hand and arm. This is usually quite rapid and may readily be

[1]The difference between records made with the automatograph and with the device figured in Fig. 1 held in the hand, is mainly one of extent of movement. The automatograph records more finely the tendency to move towards the object of attention as well as the general movements of the body. The illustrations of this article compared with those of the former show the nature of the difference. A further advantage of the automatograph is that it rests the arm.

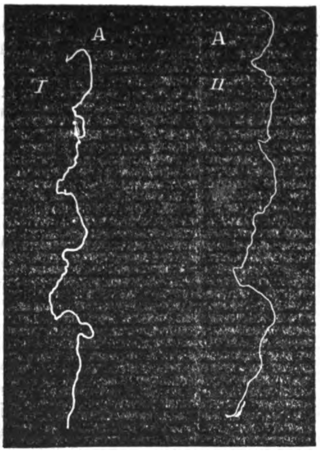

Fig. 9. ↑ Thinking of a building. Standing ↑. ↑ I, left hand; II, right hand; both holding record near the body; time, 35 seconds; records reversed.

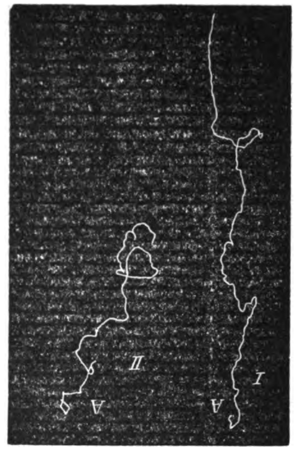

Fig 10. ↑ Thinking of a building. Facing ↓ far out. ↑ I, left hand held extended ↓ II, right hand held close to body; each hand holds record; time, 35 seconds; records reversed.

Fig. 11. ←— Counting metronome. Record vertical. Facing ←—. Time, 20 sec. Pencil held in extended right hand.

recorded; an illustration is given in I, Fig. 12. If in this position the attention is directed forward, we obtain a resultant of the two tendencies, as is shown in the diagonal curve of Fig. 11. Fig. 12

further illustrates an interesting point similar to that illustrated in Fig. 5. In curve I the attention is directed downwards, which quickens, though probably not considerably, the natural tendency for the hand to fall; in curve II the attention is directed to a point overhead, and we observe that this tendency almost exactly balances the effect of the natural fatigue and thus yields this peculiarly interesting result.

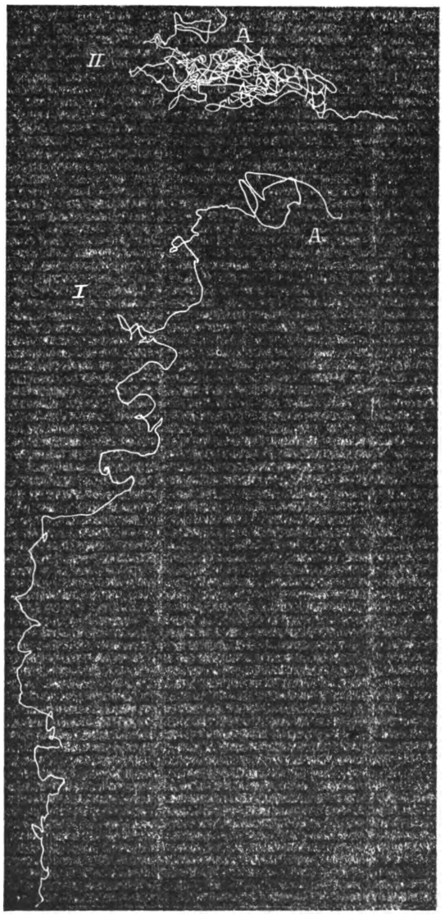

Fig 12. I, ↓ Record-plate vertical. Thinking of one's feet; time, 45 seconds. II, thinking of ↑ a point overhead; time, 45 seconds.

Our attempts to utilize this method for measuring the different degrees of attractiveness of different senses or sense-impressions have not been very successful; and this is mainly due to the great

variability of the result. We have a few illustrations of the difference in question of sufficient interest to reproduce. In Fig. 13

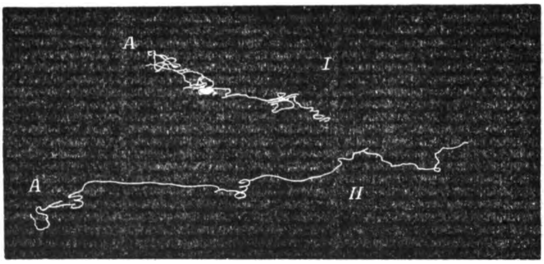

Fig. 13. →. I, counting metronome. Automatograph, facing →. Time, 35 seconds. → II, counting pendulum. Automatograph, facing →. Time, 25 sec.

curve I represents the movement of the hand while the subject was counting the strokes of a metronome for 35 seconds; the movement is towards the object of attention, but is slight. Curve II represents the movement where a pendulum is substituted for a metronome, a visual for an auditory impression. In this case the usual impression claims the attention more strongly than the auditory; and this corresponds with the subject's analysis of his mental processes. The subject is a noted American novelist and describes himself as a strong visualizer and in general an eye-minded person.

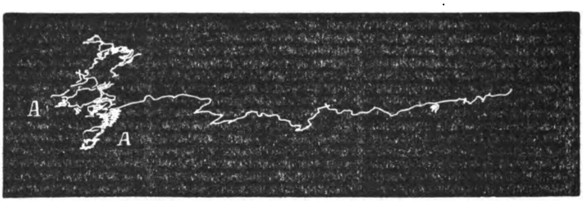

Fig 14. →. Facing →. Hand on automatograph. From A to A′; reading colors; 35 seconds. From A′ on, counting pendulum, 25 seconds.

In Fig. 14 the subject was asked to call the names of patches of colors fixed to the wall opposite him; he did this uncertainly for 35 seconds, his hand moving from A to A′; at this point the counting of a pendulum was substituted for the reading of the colors with a markedly different result, the hand moving directly and rapidly towards the pendulum. Upon examination it proved that the subject's color vision was quite defective, so that the colors did not hold his attention, while the pendulum did. The difference is too marked to be accidental, and is certainly most interesting.

An interesting problem upon which further research is contemplated is the correlation of types of involuntary movements with temperament, age, sex, individual, health and disease, and the like. A few observations were made upon children; they show at once the limited control children possess over their muscles and a similar difficulty in fixing the attention as required. They thus yield an exceedingly irregular result, showing very extensive and coarse

movements, usually towards the object of attention, but with great oscillations. Fig. 15 may serve as a type; in 35 seconds the hand

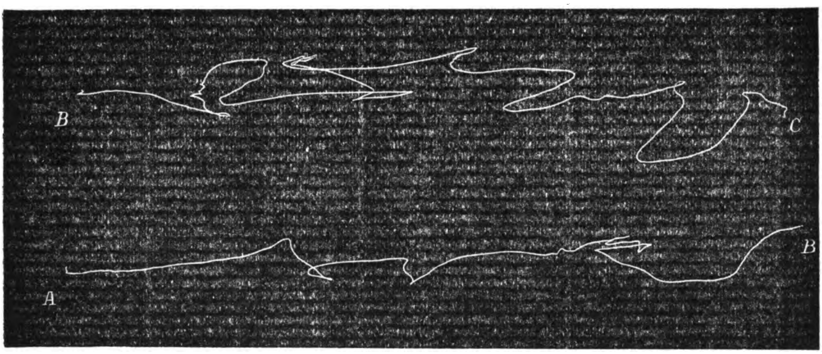

Fig. 15. ⟶ Hand on automatograph. Facing ⟶, counting pendulum. Time, 35 seconds. The record from B′ to C is continuous with that of A to B. The subject, a child of eleven years.

has moved by large skips *seven* inches toward the pendulum, the oscillations of which the child was counting. The difference between this record and those obtained upon adults is striking enough to warrant further study.

Much attention has recently been paid to the subject of automatic writing; in this the subject unconsciously gives indication not of the direction but of the nature and content of his thoughts. We made a few attempts to obtain such upon the automatograph, but entirely without success; we asked the subject to think of a certain letter or simple geometric figure and examine the record for any trace of the outline of this letter or figure. While this method yielded no trace of such a result, it gave us a valuable "control" over the movements in which the attention was directed in a definite direction. In Fig. 16 the subject was thinking of the

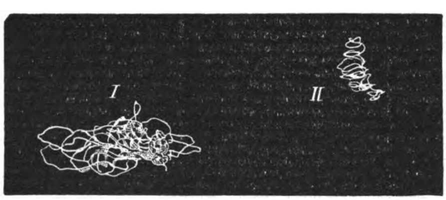

Fig. 16. Thinking of letter O. Pencil in hand; record on table. I, standing; II, sitting.

letter O; this was not thought of as in any special location and the record likewise fails to show a movement in any special direction. Two records are shown, one while standing and one while sitting, and these will show the difference in the general and local movements in the two cases; the subject is the same as in Figs. 4-12. This adds the corroboration of a negative proof to the general interpretation of the results.

FURTHER STUDIES OF CLASSIFICATION-TIMES.

Frederick E.
With the assistance of GEO. W. MOOREHOUSE, [redacted] BOLTON and E. T. JOHNSTON.

This term was applied in the last series of these studies (this Journal, Vol. IV. p. 411) to the time occupied in referring a word or object to its class. The special problem studied related to the time required for answering by the word "noun," "verb" or "adj." (for adjective) when one of ten known nouns, verbs or adjectives was called; i. e., to refer a familiar word to its proper part of speech. This was done when the words were either *nouns or verbs, nouns or adjectives, verbs or adjectives, and nouns, verbs or adjectives.* The times were measured by aid of a "mouth-key," in which the release of a bit of wood held between the teeth started or stopped a chronoscope. The natural opening of the teeth in speaking thus served to mark the limits of the time measured. A simple reaction was obtained by answering always with the sound "*eh*" (violent explosive) when the voice of the observer was heard; the action of repeating the word heard was regarded as involving essentially all the steps involved in the classification-time, except that needed for mentally making the classification. In order to bring our former results in direct comparison with those about to be described, we repeated the same set of reactions upon F. E. B. and E. T. J. as were last year made upon the other two observers. The following table gives (in $\sigma = .001$ second) the simple reaction-time; the repetition-time; the classification-time of nouns and verbs, of nouns and adjectives, of verbs and adjectives; the average of these three; and the classification-time of nouns, verbs and adjectives. Each entry represents the average of about 12 sets of 20 reactions each. The lowest line gives the general average of all the results.

Subject.	Simple Reaction.	Repetition.	Noun-verb.	Noun-adjective.	Verb-adjective.	Average of N.-V., N.-A. and V.-A.	Noun-verb-adjective.	Difference between Simple and Average of N.-V., N.-A. and V.-A.	Difference between Repetition and Average of N.-V., N.-A. and V.-A.	Difference between Average of N.-V., N.-A., and V.-A., and N.-V.-A.
J. J.	190	367	599	595	593	596	667	406	229	71
G.W.M.	195	280	628	597	679	635	678	440	355	43
F. E. B.	201	364	579	623	604	602	663	401	238	61
E. T. J.	179	366	612	619	611	614	684	435	248	70
Aver.	191	344	604	608	622	612	673	421	268	61

From this table we may gather that it takes 191σ to signal by a vocal reaction that a sound has been heard; 344σ to repeat a spoken word; the difference between these 153σ for the processes of distinguishing the sound and calling into play the proper vocal utterance; 612σ to refer a spoken word to one of two grammatical

classes, there being no difference amongst the three pairs of parts of speech used; 673σ or 61σ additional to do this when the spoken word may be one of three classes of ten each; and 268σ for the mental process of deciding and recalling what the appropriate part of speech is.[1]

Our next step was to substitute for the grammatical classes the three classes of *animals*, parts of the human *body*, and articles of *dress;* as before, ten distinctive and familiar monosyllabic words were chosen as follows: *dog, fox, hen, pig, cow, bee, snake, goose, goat, horse; eye, ear, nose, mouth, head, arm, hand, foot, tooth, thumb; hat, cap, coat, vest, glove, shoe, boot, tie, cuff, shirt.* We answered "*an*," when the name of an animal was spoken; "*bod*," for parts of the body; and "*dress*," for articles of dress. As before, we classified these words when they were selected from *animals* and parts of the *body*, from *animals* and *dress*, from *body* and *dress*, and from *animal, body* and *dress;* and in all respects repeated for these classes what he had done for the grammatical ones. Our results are embodied in the following table, in which each entry is the average of 12 sets of 20 reactions each:

Animals, Body, Dress.—*Verbal Series.*

Subject.	Animal-body.	Animal-dress.	Body-dress.	Average of A.-B., A.-D., B.-D.	Animal-body-dress.	Difference between Simple and Average A.-B., A.-D., B.-D.	Difference between Repetition and Average of A.-B., A.-D., B.-D.	Difference between Average of A.-B., A.-D., B.-D., and A.-B.-D.
J. J.	747	768	693	736	809	546	369	73
G. W. M.	567	575	551	564	637	369	284	73
F. E. B.	724	709	683	705	783	504	341	78
E. T. J.	658	694	673	675	737	496	309	62
Aver.	674	686	650	670	741	479	326	71

Several conclusions drawn from the former table reappear in this one. The three kinds of classifications with two classes take about equal times; the additional time needed to make the same classification when the word may be one of three classes is about the same. Further, the process of classifying words as *animals, body* or *dress* is longer than classifying them as parts of speech for three subjects, for J. J. by 140σ, for F. E. B. by 103σ, for E. T. J. by 61σ, but 71σ shorter for G. W. M. G. W. M. experienced

[1] It is also noted that our additional research entirely confirms the conclusions drawn in the last study, viz., that the results for J. J. are more typical than the others. The peculiar difficulty of G. W. M. to distinguish verbs from adjectives still affects the results. But the main irregularities in our former results, due to irregularities of practice, do not reappear, owing to the fact that care was taken that the various kinds of reaction be equally distributed throughout the work.

unusual difficulty in naming the parts of speech of words, and found the classification with *animal, body, dress* somewhat easier than the others; it is, perhaps, fair to regard the average of the other observers 102σ (or an increase of 17%) as representing the increased difficulty involved. It is easier upon hearing the word dog, to recall and say that *dog* is a *noun* than that *dog* is an *animal;* which would in turn indicate that we have been better schooled in recognizing the parts of speech of words than in recognizing the more or less natural classes into which the objects denoted fall.

Our three groups of ten words each were chosen with reference to easy pictorial illustration, for our design included the classification of the pictures of these objects as well as of the spoken words. For this purpose pen and ink drawings of the thirty objects were prepared and fastened upon small slips of glass; the drawings were of a uniform size, the extreme outlines being contained within a circle 35 mm. in diameter. A frame was made in which ten of these pictures could be mounted in a carriage and moved along horizontally in back of a shutter such as the photographers use. This shutter consisted of two wings in back of an opening of adjustable size; a pressure on an air bulb withdrew the wings from the opening in the usual way, and in so doing established an electric circuit by which the chronoscope was started. By the aid of a series of spring stops and a weight to move the carriage, we could conveniently and quickly bring any one of the ten pictures behind the opening in the shutter. The reaction was made as before by speaking the appropriate class name with the mouth-key. After each ten reactions the pictures were changed and two series of ten reactions each constituted a set. The following table, arranged similarly to the former tables, gives the results of our "pictorial" series. Each entry under "simple" and "naming" represents the average of 12 sets of 20 observations each; each entry under the other columns of 8 such sets.

ANIMAL, BODY, DRESS.—*Pictorial Series.*

Subject.	Simple.	Naming.	Animal-body.	Animal-dress.	Body-dress.	Average of Animal-body, Animal-dress, Body-dress.	Animal-body-dress.	Difference between Simple and Average of A.-B., A.-D., and B.-D.	Difference between Average of A.-B., A.-D., and B.-D. and A.-B.-D.
J. J.	202	484	524	526	570	540	603	338	63
G. W. M.	214	522	547	494	532	526	605	312	79
F. E. B.	235	563	563	561	577	567	639	332	72
E. T. J.	185	558	545	507	524	526	589	341	63
Aver.	209	532	545	522	551	539	608	330	69

Again it appears that the classification of a word into any one of the three pairs of classes requires about equal times, and that the

additional time to do this for one of three classes remains the same, about 70σ.

The chief result of a comparison of this with the former table is the fact that it takes *less time to classify a picture than a word;* less time to recall and say that "dog is an animal" when the picture of a dog is shown than when the word is spoken. While the classification in the verbal series (for two classes) requires 670σ, in the pictorial series it requires only 539σ, or 131σ less; for classification into three classes, 741σ and 608σ, or 133σ less. It is fairer to take account of the differences in the simple reaction-time of the verbal and pictorial series, 191σ and 209σ; and thus the difference in the mental processes of classification is greater by 18σ than the differences just given.

In the verbal series we found reasons for regarding the time of repeating a spoken word as involving all the processes of classifying the word except the act of recalling the classification (see Vol. IV. pp. 412–413); the pure mental classification time for the grammatical series (two classes) would thus be 268σ, for the verbal animal-body-dress series 326σ. In the pictorial series we were unable to devise any means of making this elimination, and so cannot say how much of the difference between the simple reaction and the entire classification-time, 330σ, is taken up by the process of recognizing and indicating the recognition of the picture, how much by the recalling of its class name. There are strong reasons for believing that very much the greater portion of the 330σ is taken up in the mental classification process.

It is further of interest to compare the process of classifying with that of naming. Is it a more complicated process, upon seeing the picture of a dog, to say "dog" or to say "animal"? Do we first recognize the lines as representing a dog, and then decide that a dog is an animal, or do we at once recognize the drawing as that of an animal? We are able to give but an imperfect answer to this question. For J. J. it is easier to name than to classify, and the time is shorter by 56σ; for G. W. M. and F. E. B. there is practically no difference; for E. T. J. naming requires 32σ longer. The inferences from these results are that the two processes are about of equal complexity, that it is unlikely that the specific recognition of the class to which the object belongs includes the recognition of the individual object, and that the processes may be different in different persons.

We have already noted that the three pairs of distinctions of classes are of equal difficulty; it is further of interest to ascertain whether it is easier to pronounce a word a noun, verb or adjective; a name, a picture, that of an animal, of a part of the body, or of an article of dress. The following table gives the data for this decision, and by noting the numbers in bold type we see that on the average of all cases in which a word was pronounced a noun, the time required was 616σ, for verbs 627σ, for adjectives 651σ; for animals 695σ, for body 690σ, for dress 698σ; for pictures of animals 555σ, for body 568σ, for dress 564σ. In each class the three types of classification are thus of practically equal difficulty. The increase of time needed to pronounce a word an adjective above that needed to pronounce it a noun is of note, however, amounting to 35σ in the average and appearing in each of the four individuals. The individual records agree well with their average. The table will doubtless be clear without further comment:

Subject.	Noun.				Verb.				Adjective.			
	N.-A.	N.-V.	N.-V.-A.	Aver.	N.-V.	V.-A.	N.-V.-A.	Aver.	N.-A.	V.-A.	N.-V.-A.	Aver.
J. J.	604	594	631	610	594	568	651	604	596	618	698	637
G. W. M.	620	568	657	615	636	669	675	660	627	689	701	672
F. E. B.	594	626	610	610	564	604	652	607	620	604	728	651
E. T. J.	609	612	665	629	615	613	683	637	626	607	704	646
Average.	607	600	641	616	602	613	605	627	617	629	708	651

Subject.	Animal.				Body.				Dress. (Words.)			
	A.-B.	A.-D.	A.-B.-D.	Aver.	A.-B.	B. D.	A.-B.-D.	Aver.	A.-D.	B.-D.	A.-B.-D.	Aver.
J. J.	744	764	806	771	750	687	806	748	772	701	814	762
G. W. M.	566	547	628	580	568	549	632	583	603	555	651	603
F. E. B.	723	739	743	735	725	683	804	737	679	683	803	722
E. T. J.	657	680	742	693	659	667	746	691	708	679	723	703
Average.	674	682	730	695	675	647	747	690	690	655	748	698

Subject.	Animal.				Body.				Dress. (Picture.)			
	A.-B.	A.-D.	A.-B.-D.	Aver.	A.-B.	B.-D.	A.-B.-D.	Aver.	A.-D.	B.-D.	A.-B.-D.	Aver.
J. J.	521	517	581	540	527	569	619	572	535	571	610	572
G. W. M.	557	501	592	550	537	514	620	564	485	550	592	542
F. E. B.	561	562	622	582	567	576	635	593	560	578	661	600
E. T. J.	554	495	592	547	536	528	589	551	519	522	587	543
Average.	548	519	597	555	542	547	616	568	525	555	612	564

It is also true that it takes practically the same time to name a picture of an animal, a part of the body, or an article of dress. This appears in the following table:

Naming Pictures of Objects.

Subject.	Animal.	Body.	Dress.	Average.
J. J.	488	469	494	484
G. W. M.	530	513	524	522
F. E. B.	573	569	546	563
E. T. J.	557	558	558	558
Average.	537	531	527	532

The final average (532σ) represents the average of 960 reactions, animals being named 340 times, parts of the body 266 times, and articles of dress 354 times. We also append a statement of the number of errors, i. e., in which a word or picture was referred to a class not its own. Our data for nouns, verbs and adjectives relate only to F. E. B. and E. T. J.; for these the numbers of errors in percentage of observations taken are as follows:

SUBJECT.	N.-A.	N.-V.	V.-A.	N.-V.-A.	Average.
F. E. B.	1.67	6.17	6.17	2.92	4.27
E. T. J.	2.92	1.67	1.67	1.25	1.88
Average.	2.30	3.92	3.92	2.09	3.08

For the verbal series the errors in percentage are:

SUBJECT.	A.-B.	A.-D.	B.-D.	A.-B.-D	Average.
J. J.	7.92	5.42	3.33	5.83	5.77
G. W. M.	8.92	12.92	12.92	14.54	12.29
F. E. B.	5.00	2.92	5.42	2.50	3.96
E. T. J.	4.58	6.17	5.00	1.45	4.38
Average.	6.61	6.86	6.67	6.08	6.60

Average of F. E. B. and E. T. J., 4.17.

For the pictorial series the errors in percentage are:

SUBJECT.	A.-B.	A.-D.	B.-D.	A.-B.-D.	Average.
J. J.	1.25	3.75	0.63	0.67	1.56
G. W. M.	10.00	0.63	3.75	8.12	5.62
F. E. B.	0.63	1.25	1.87	1.25	1.25
E. T. J.	0.63	0.63	1.87	2.50	1.41
Average.	3.13	1.57	2.03	3.12	2.46

Average of F. E. B. and E. T. J., 1.33.

It appears that the percentage of error is smallest for the pictorial series, largest for the verbal series, and intermediate for the grammatical series. The individual difference that should be noted is the large number of errors of G. W. M., which is undoubtedly related to the shortness and mode of his reactions. The order of the four subjects regarding their liability to error is the same in the

verbal and pictorial series. It is also of interest to inquire whether the average time of these erroneous reactions is markedly different from the time of the correct ones. In the following table the average time of the erroneous reactions is given in percentage of the corresponding average correct reactions; and the results show, on the whole, no appreciable difference between the two. In the erroneous reactions there is probably a greater variation than in the correct ones:

	J. J.	G. W. M.	F. E. B.	E. T. J.	Average.
Grammatical Series.	. . . . .	. . . . .	94.8	119.5	107.1
Verbal Series	108.6	97.6	99.9	103.1	102.3
Pictorial Series.	97.2	105.3	107.0	97.2	101.7

ON THE PERCEPTION OF SIMULTANEOUS SENSE-IMPRESSIONS.

With the assistance of GEORGE W. MOOREHOUSE, Fellow in Psychology.

The error in indicating with which one of a series of visual impressions an auditory or other impression seems simultaneous, was first noted by Wundt. He studied it by having an index rotate in front of a graduated disc at a constant rate, or again by having it oscillate with a pendular movement, and noting to what stroke of the disc the hand seemed to be pointing when a bell sounded. The actual moment of the sound was determined by the observer by moving the pendulum slowly across the disc and the error in time was then calculated by mathematical formula. With this apparatus Wundt established that it takes many separate judgments before one is ready to make one's decision; that the error is very variable; that the error for almost all the rates of movement used is negative, i. e., the time at which the bell is said to have rung precedes the time of its actual ringing; that this error decreases as the speed increases, until it becomes positive; that the error increases in the accelerating portion of a pendular movement, and decreases in the portion of diminishing velocity; that the error with the constant motion disappears when one division[1] of the disc corresponded to $\frac{1}{36}$ of a second ($=28\sigma$) and the interval between successive sounds of the bell is one second; and finally the very important fact that the determination was considerably under the control of the will of the observer, and was influenced by the direction and nature of the attention.

Wundt also experimented with more than two simultaneous impressions, but his results on this point need not now be considered.

Tschisch (Wundt's Studies II. 603-634) has contributed an elaborate research, working with the same apparatus, but his main results are concerned with the determination of the error with several simultaneous impressions. Reference to his results will be made later on. Both Tschisch and Wundt connect with their results an elaborate theoretical interpretation.

[1] This division refers to the smallest portion of the divided circle taken into account in the subject's judgment; Wundt's apparatus as figured has a mark for every two degrees, but it is to be inferred that he judged only to the nearest ten degrees. The importance of this point is the subject of discussion below.

For several reasons a reinvestigation of the fundamental factors of these interesting phenomena seemed desirable; the accepted interpretation of the error as the time needed for the reception and elaboration of the perception (*Complication einer Vorstellung*[1]) seemed questionable; the dependence of the error upon the apparatus as well as upon the mode of judgment seemed not to have been sufficiently regarded. The phenomenon, when reduced to its simplest terms, may be thus described: There is a series of sense-impressions following one another in a recognizable order and the members of which are distinguished from one another both in time and by some other characteristic; a disparate and momentary sense-impression is interposed at some moment unknown to the subject, and he must determine with which one of the series of impressions the disparate impression seemed to coincide. In order to take note of small errors, it is necessary that the successive members of the series of sense-impressions be rapidly distinguished; and sight and hearing alone, therefore, are available for this purpose. It is true that we can distinguish both these and other sense-impressions by the artificial device of counting, but this process is too slow and absorbs too great a share of the attention to be here available. Sight is decidedly the preferable sense by reason of its superior power of taking in a large range of impressions at once; and in many ways the most convenient visual impressions are the divisions of a divided circle. The place of a given mark in the circle is readily determined when each fifth or tenth mark is differentiated from the others; the circle used by Wundt has a short stroke for each two degrees and a larger stroke for each ten degrees, and this division we have used in our experiments. A point travels along this visual scale, and for the interposed impression the stroke of a bell or an electric shock on the finger is most convenient. Our problem then is simply this: Where upon the divided circle was the moving point when the bell sounded or the shock was felt? The most important factor in this decision is obviously the accuracy with which the subject is required to decide; i. e., whether he is to determine the point when the bell sounded to the nearest ten, five, two or one degree; this is the one point that must be determined before the observations can proceed, and indeed must be considered in the preparation of the divided circle. And yet it is a surprising fact that this is the one point upon which former observers have been most reticent; one can only infer it, and that not too certainly from the apparatus used. Wundt, in his observations with an index revolving at a constant rate, judged to the nearest ten degrees; Tschisch apparently judged more accurately, and if by a division (Theilstrich) he means a division of Wundt's apparatus as figured, he judged to the nearest two degrees. The accuracy of the judgment with a given rate of movement is dependent upon the size of the divisions; after a certain velocity has been reached, we can no longer distinguish the several positions of the moving point. The larger the disc, the higher the speed as measured by the time of one revolution at which an interval of a given number, say two degrees, may be distinguished. Wundt's disc for constant movement was but slightly over six inches in diameter, and thus it is clear why he could judge only to the nearest ten degrees; in the apparatus of Wundt used by Tschisch, the disc is about 8½ inches in diameter.

[1] Tschisch has elaborated and Wundt has endorsed this interpretation as applied to more than two simultaneous impressions; and a table is given indicating the time of the several higher kinds of reception and fusion of perceptions (Tschisch p. 633; Wundt, 3rd Ed. II. p. 341).

To obtain a greater range of velocity of movement we used a much larger disc, 22 inches in diameter, divided by short strokes into two degrees, and by a longer one for every ten degrees. We judged not only to the nearest stroke, but also whether the point stood on or between two strokes when the bell sounded, i. e., we *judged to the nearest degree.* The distance on our disc between two strokes (two degrees) was 9.75 mm., or about $\frac{3}{8}$ of an inch; in Wundt's disc this distance was only 3.84 mm., about $\frac{3}{20}$ of an inch. Furthermore, to secure ease of reading, the markings were plain and bold, in black ink upon white card-board, and the index was blackened and tapered to a readily visible point.

In almost all previous determinations, the index moved with a pendular movement; while it is interesting to observe the effect of the change of velocity upon the error of judgment, it is certainly important to have determined the error for a constant rate as a standard of comparison; and to regard the rate at the base of a pendular oscillation as equivalent for this purpose to a constant rate is not free from objection. In all our observations, the index traveled over the disc at a constant rate.

This disc was glued to a board, 22 inches square, and the whole mounted in a vertical position; through a hole in the centre of the disc, the axle bearing the index projected and this index could be set in any position and then fastened by a thumb-screw. The mechanism by which the index was rotated was the clock-work of a clinostat. This apparatus was admirably adapted to our purpose and admitted of a great range of velocity. On the axle, behind the disc, was fastened a small wire, the end of which just dipped into a mercury drop, and thus in each revolution established an electric circuit. By this connection, a bell could be struck or an electric shock given to the finger, and by the setting of the index the point at which this occurred was charged. Moreover, a switch in each circuit enabled the observer to introduce the sound or the shock at any desired moment; this is important, as no judgment should be made until the clock-work has obtained its full and constant rate. The subject sat at a convenient distance before the disc, the latter concealing from his view all the mechanism by which the index was rotated as well as the bell, and called out the positions at which the sound or shock seemed to come.

A further point in the method of observation is of importance. The judgment in which the subject has any confidence is formed only after several observations, the point at which the impression was interposed shifting with each observation. There are two natural methods of recording the error; the one is to take the *average of all* the observations with a given setting of the interposed impression; the other is to ask the subject to decide upon *one judgment as the final* one, and to measure the error by this, recording, however, the several observations as well. After a trial of both, we adopted the latter plan as the better. The point at which the interposed impression really occurred was readily determined by slowly moving the index (by turning with the finger one of the fine wheels of the clock-work) until the bell sounded or the shock was felt. In order to have the sound or shock as brief as possible, the mercury cup was made in the form of a narrow slit, through which the point of the wire could be made to pass at any desired angle, and to prevent the sound of the bell from continuing after it was struck, the bell was loaded with drops of wax. In some cases we found it more convenient to use a spring wire instead of the mercury drop. The rate of the index was determined by timing to the nearest second three, five or ten revolutions, according to the

rate, and the result was expressed uniformly *in the σ=.001 of a second required for the point of the index to travel over one degree of the circumference.*

The results we have thus far accumulated are in every sense provisional; the number of observations is not adequate and they are offered at the present time simply because of the interest in the methods by which they have been obtained and their explicit disagreements with previous results.

We calculate from the tables of Tschisch, that with the index moving at such a rate that 1° was passed over in 3.07 σ, the error for sound was a negative one of 64.8 σ, for touch 64.8 σ, for electric shock 72.2 σ; with a faster rate of 1° in 2.41 σ, these errors were 44.1 σ, 44.1 σ and 39.9 σ; and with a still faster rate of 1° in 1.7 σ, these errors were 20.3 σ, 20.3 σ and 20.3 σ. In all cases these are the errors at the base of the pendular movement, when the acceleration is zero; and by a negative error is meant an error in judging the interposed impression as occurring in advance of its actual occurrence. The sound was that of a bell, the touch a tap of a hammer upon the frontal surface of the last joint of the forefinger and the third kind of stimulus was an electric shock, presumably upon the finger. Tschisch does not describe his manner of recording the judgments, whether he averaged all the observations or accepted a final judgment with each test; in what way he combined errors of opposite direction and the like. The chief characteristics of his result are the large size of the errors; the decrease of the error with an increase of speed and that, too, within small range; the constancy of this error with different kinds of interposed stimuli, and the negative character of the error throughout.

In our observations, the individual variation of the results is so very great that it seems somewhat strained to attach any importance to the general average. These variations are so great that in all the observations with any one rate of speed, observations with positive as well as negative errors occur. Furthermore, within the range of velocities studied by Tschisch, we can distinguish no constant tendencies at all, and within the very much larger range of velocities at our disposal nothing that could be dignified by the name of law appears. Taking all our observations together we find for the sound:

Rate,	1° in 2 to 4 σ.	1° in 4 to 6 σ.	1° in 6 to 9 σ.	1° in 9 σ or more.
Error,	—10.6 σ	+7.0 σ	+5.9 σ	+1.8 σ

These numbers are based upon 120 observations in all.

With the electric shock as the stimulus the results are:

Rate,	1°=2 to 4 σ.	1°=4 to 6 σ.	1°=6 to 8 σ.	1°=8 σ or more.
Error,	—15.8 σ	—3.5 σ	—6.4 σ	—45.1 σ

These results are based upon 130 observations in all.

These results, though entirely provisional and without much significance, owing to the great individual variations, yet are opposed to all the four main results of Tschisch's experiments. What this opposition means, it would be premature to say. But three points, further, need be noticed: (1) the relative constancy of the results when calculated without regard to their positive or negative characters; (2) the difference of individual observers in these observations; (3) the tendency of the several individual judgments in a single observation. With regard to (1) it is only necessary to indicate this fact: this error is not far from 30 σ for the sound, and from 40 σ for the electric shock, independently of rate. (2) We have tested a sufficient number of individuals to make great differences in the size and direction of the error, but not enough to describe them in quantitative terms. With regard to (3) we can only say that with

some individuals the index nearly always moves to a later point of the disc with successive sounds of the bell or shocks on the finger.

We did not confine our studies to this method of observation, but devised several others, to the description of which we may now turn.

We arranged a method by which a series of auditory impressions could be substituted for the visual ones. This arrangement requires the services of three persons; in one room there is the subject with his finger on the "shock key," listening to the reading of the observer; the observer, in reading, speaks into the mouth of the transmitter of a telephone, and at the other end of the telephone in another room sits the recorder; there is, further, an arrangement by which, either automatically or at the desire of the recorder, the shock may be given and the telephone circuit broken. The subject notes at what word he was listening when the shock came and the recorder records the last word heard before his telephone was cut out of the circuit. A simpler mode of observation consisted in connecting the shock circuit with the telephone circuit and noting between what words the slight sounds accompanying the making and breaking of the shock circuit came. We then measure the rate of speaking and calculate the error in σ. In reading from a book, the subject does not know what is coming; but in observing the movements of an index before a disc, the sequence of impressions is foreknown. We can secure the latter conditions for hearing by counting, or by speaking the alphabet. A further variation consists in having the subject himself count aloud or read aloud from a printed page; but this is not so serviceable as the other form of experiment. The result of all our experiments with the auditory series may thus be expressed: The error in indicating the place of a shock in an auditory series is less than one of the smallest units of time (the time needed to speak one word or one syllable) that we could take into account in the observations. The quickest sense-impressions that one can follow by ear is counting from one to ten repeatedly, but this can hardly be done more rapidly than seven per second; our result then simply shows that the error is rarely as large as one-seventh of a second $= 143\ \sigma$. It is further to be noted that the recorder's method of noting the error is not as objective as is to be desired. In noting the place of the two clicks in the auditory series, there is some though less opportunity for the same error of time-location as in placing the position of a shock in the same series. The experimentation is difficult, the results indicating that the ear cannot differentiate and locate the sequence of impressions with sufficient rapidity to permit of the detection of the error under consideration.

As a further contribution to the influence of the apparatus and mode of judgment upon these errors of location in time, we altered one of the most important conditions of all former experiments: instead of having the disc stand still and the index moved, the reverse was done. This was accomplished as follows: Upon a revolving drum was fastened a sheet of paper with various lines of letters, words and numbers written upon it with a type-writer; the drum was in a horizontal position, but to bring the letters in a vertical position and to have them pass across the field of vision from right to left (and thus be read from left to right) as well as to have but one line, or rather as much of one line as one could see, in sight at any one time, two mirrors were appropriately placed at the end of a shallow box, through which the subject read. A fine thread placed in front of one of these mirrors served as an index, the subject judging what letter or number was opposite the thread when

the bell sounded or his finger received the shock. As before, the drum moves at a constant rate, and a final judgment is recorded after several individual observations. We judged always to the nearest letter or number (there were 135 in a line), and in some instances we attempted judgments between or upon letters or spaces, that is, to the nearest half letter. It will be seen that a letter corresponds to $2\frac{2}{3}°$ or a half-letter to $1\frac{1}{3}°$. We used 12 different lines; lines I. and II. were continuous words from a story for children; line III., a series of detached monosyllabic words; line IV., miscellaneous numbers between 20 and 100; line V., the numbers in regular order from 20 on; line VI., the numbers advancing by 7 from 12 to 100, 11 to 100, 13 to 100; line VII., letters of the alphabet in chance order; line VIII., a line of verse; line IX., of prose; line X., the same line of prose, but in reversed order; line XI., a line of German; line XII., a scale of short uniform marks with every fifth mark heavier and numbered to correspond exactly with the divided circle with the rotating index.

Let us consider line XII. first, as that allows of most direct comparison with former results. With this method of judging a much slower rate is necessary; the circle is much smaller, about 4 inches in diameter; it is more difficult to read the lines while in motion, and a smaller portion of the circle is visible at any one time.

With the bell our results are :

Rate,	1° in 20 to 24 σ.	1° in 24 to 28 σ.	1° in 38 to 42 σ.	75
Error,	—27.6 σ	—20.7 σ	—28.8 σ	observations.

With the electric shock, the results are :

Rate,	1° in 20 to 24 σ.	1° in 24 to 28 σ.	1° in 32 to 50 σ.	60
Error,	—50.7 σ	—23.2 σ	—54.4 σ	observations.

The great variability of the results is again a striking factor, though they are almost uniformly negative. There is no definite connection suggested between rate and error, and the error is different with the bell and with the shock.

The other eleven lines were arranged to furnish material for the study of the effect of the different kinds of visual series upon the error. Most of the observations were, therefore, made with one rate of speed, about 1° in 20 σ. If we divide the lines into those containing continuous words, I., II., VIII., IX., XI., those containing detached words or letters, III., VII., X., and those containing numbers, we find as the general average error of the first set $+2.2\sigma$, of the second $+7.0\ \sigma$, of the third, $0.0\ \sigma$. There is no difference of note between the results of bell and shock. The small error and great variability of the results are again the marked characteristics; the averages have thus little significance. But one further result is worthy of notice. If, instead of recording simply the final judgment, we record all the individual answers and enter their average as a result, then the error seems to be larger; we can make such a comparison for the "shock-stimuli," though the two sets of results were not taken at the same rate (the "average" judgments being at about half as rapid a rate as the "final" ones). In the first case the average error is $4.1\ \sigma$ and in the second case $35.7\ \sigma$. Attention is again directed to the provisional character of the results throughout, and to the fact that our main objects are the analysis of the factors involved in these observations, the indication of the dependence of the results upon method, apparatus and mode of judgment, and the recording of the absence of agreement of our provisional results with those obtained by other observers.

The Psycho-physic Series Applied to Lifted Weights.

With the assistance of Walter D. Brown.

The method of the psycho-physic series presents to the subject a range of sensation intensities—usually successively one at a time—and requires him to assign each sense-impression to one of a given number of divisions or magnitudes. By this method we may compare the assigned magnitudes of the stars with their photometric intensities and determine whether the subjectively equal different magnitudes (arithmetical series) correspond to a similar objective series or—as the psycho-physic law demands—to a series of equal ratios (geometrical series). In former contributions (Vol. I. pp. 112-127, Vol. III. pp. 44-79, Vol. IV. pp. 213-219) the close correspondence of estimated star-magnitudes with the law was shown, and the method applied with varying results to sensations of visual and tactual expression, to the time-sense and to the motor-sense. A comparison of all the results suggested the generalization that the law probably holds of sensations that are appreciated *en masse*, without conceiving them as divided into units, on a general unanalyzed impression.

Our present study applies this method to the sensations obtained in lifting weights in the palm of the hand, and this includes the sense of muscular contraction as well as the pressure-sense of the palm. Sixty weights were prepared, the lightest weighing 12 grms. and the heaviest 795 grms.; the intermediate weights corresponded to the average of an arithmetical and geometrical series inserted between these limits; in this way the selection of weights, while from the subject's point of view essentially a matter of chance, favored one result no more than the other. A set consisted of 60 observations, each weight being assigned once to a magnitude. The subject, without distinctly seeing the weight, lifted it up and down in his right hand and assigned it according to his sensations, to one of six magnitudes or classes. The lightest weights were grouped as Class I., the heaviest VI. The order of the weights was determined by chance. The weights were made by packing cylindrical boxes $3\frac{1}{4}''$ high and $1\frac{5}{16}''$ in diameter with leaden discs cut to fit the inside of the box and supplemented by felt discs, cotton and shot; the weight was equally distributed throughout the box and were all alike in appearance, being marked by a letter on the bottom. Two sets (120 judgments) were taken upon seven subjects and four sets (240 judgments) upon three subjects. The average weight in grammes and the results of all the weights assigned to each of the six magnitudes or compartments I., II., III., IV., V. and VI. by each of the new subjects is given in the following table, the last line giving the average of all:

Subject	No.	I	No.	II	No.	III	No.	IV	No.	V	No.	VI	No.
J. H. T.	120	39.4	23	103.2	23	175.4	14	249.2	14	370.1	18	619.4	28
F. S.	120	43.9	23	92.3	23	178.9	16	257.2	14	400.8	21	640.4	23
J. H. D.	120	25.8	13	71.2	17	146.0	28	286.0	22	438.5	20	664.3	20
F. E. B.	120	27.3	14	74.6	19	131.6	18	240.4	24	391.8	21	640.2	24
E. P. S.	120	23.6	19	91.1	20	181.5	28	323.4	17	461.7	19	696.2	17
C. M. R.	120	34.5	20	97.6	23	170.7	18	285.2	23	433.9	15	660.0	21
E. T. J.	120	40.5	26	127.8	29	239.3	20	369.2	16	526.2	14	676.1	15
G. W. M.	240	27.0	30	58.0	41	148.2	39	232.6	36	370.2	45	624.3	49
W. D. B.	240	29.5	30	75.8	34	145.1	46	255.8	44	408.9	40	643.8	46
J. J.	240	28.2	30	76.2	39	151.4	47	266.1	45	422.6	28	620.1	51
Average	1560	32.0	228	86.8	268	166.8	274	276.5	255	422.5	241	648.5	294

In the next table are given for each subject, in the upper line, the successive *differences*; in the lower line, the successive *ratios* between the average weights of neighboring magnitudes; in the last three columns are found the averages of these differences and of these ratios, the average deviation of the several differences and of the ratios from their mean (expressed in percentage), and the ratios of these percentages of deviation to one another. In the lowest lines of the table, similar results are given for the general average of all.

Subject	I-II	II-III	III-IV	IV-V	V-VI	Average	Average Deviation	Ratio
J. H. T.	63.8	72.2	73.8	120.9	249.3	116.0	47.7%	1: 2.51
	2.62	1.70	1.42	1.49	1.68	1.79	19.0%	
F. S.	48.4	86.6	78.3	143.6	239.6	119.3	48.4%	1: 3.56
	2.10	1.94	1.44	1.56	1.60	1.73	13.6%	
J. H. D.	45.4	74.8	140.0	152.5	225.8	127.7	42.4%	1: 2.37
	2.76	2.05	1.96	1.53	1.52	1.96	17.9%	
F. E. B.	47.3	57.0	108.8	151.4	248.4	122.5	50.4%	1: 2.77
	2.73	1.77	1.82	1.63	1.63	1.97	18.2%	
E. P. S.	67.5	90.4	141.9	138.3	234.5	134.5	35.7%	1: 1.89
	3.90	1.99	1.77	1.43	1.51	2.12	18.9%	
C. M. R.	63.1	73.1	114.5	148.7	226.1	125.1	39.8%	1: 1.93
	2.82	1.75	1.67	1.52	1.52	1.85	20.6%	
E. T. J.	87.3	111.5	129.9	157.0	149.9	127.1	17.4%	1: 0.62
	3.15	1.87	1.54	1.43	1.28	1.85	28.3%	
G. W. M.	31.0	90.2	84.4	137.6	254.1	119.5	53.8%	1: 2.97
	2.15	2.55	1.60	1.59	1.69	1.91	18.1%	
W. D. B.	46.3	69.3	110.7	153.1	234.9	122.9	46.3%	1: 2.86
	2.57	1.91	1.66	1.60	1.57	1.86	16.2%	
J. J.	48.0	75.2	114.7	156.5	197.5	118.3	39.7%	1: 2.17
	2.66	1.99	1.76	1.59	1.47	1.91	18.3%	
Average	54.8	80.0	109.7	146.0	226.0	123.3	40.6%	1: 2.04
	2.71	1.92	1.65	1.53	1.53	1.87	19.9%	

The general result is indicated in the last entry in the column of ratios; *the approximation to a geometrical series is, on the average, twice as close as to an arithmetical series; to this extent the psychophysic law is followed.*

Passing to the individual results, it is seen that all the individuals, with one exception (E. T. J.[1]), favor the geometric series, and of these nine, six approximate it more closely than the general result. If we omit the one divergent record, the general deviation from an arithmetical series becomes 43.4%; from a geometrical series, 17.8%, and their ratio as 1: 2.44. We have, then, a coarsely approximate geometric series, but one which presumably is fairly constant in different individuals.

In the application of the method of the psychophysic series to the time-sense, it was found that the first set of those subjects upon whom more than one set was taken, conformed much more closely to a geometrical series than did the following ones. The same is true of only one of the three subjects who contributed two sets to the present study. For W. D. B. the ratio of approximation to a geometrical series to that of an arithmetical series, as in his first set, 1: 5.27; in his second set, 1: 1.60; in the two combined, 1: 2.86; for G. W. M. these ratios are 1: 2.83; 1: 2.42; and 1: 2.97; for J. J., 1: 1.63; 1: 1.90; and 1: 2.17. This would indicate that practice has less tendency to change the method of judging lifted weights than of time-intervals; in the latter case the approximation to a geometric series is much closer than in the former.

It will be observed that the deviation from a geometric series proceeds, not in a hap-hazard way, but exhibits a fairly definite and constant tendency. The ratio between the average weights of neighboring magnitudes is not a constant, but decreases by smaller and smaller steps, and thus approaches a constant. The unusually high ratio between the classes or magnitudes, I.-II. is a common characteristic of such results (see this Journal, Vol. I. p. 123, Vol. IV. p. 216) and is in large measure accounted for by the fact that the number and average weight of observations falling in Class I. are affected by there being no class smaller than I., to which doubtful judgments might be assigned. The decline of the ratios was in the case of the star-magnitudes expressed by an empirical formula, making the ratio a constant multiplied by a constant times the excess of the magnitude, above a given magnitude. The ratio is expressed, not by a straight line, as it would be if it were a constant, but by a line inclined to the horizontal at a slight angle. A similar inclination, though not a constant one, is suggested by the present results.

The suggestions offered in former applications of this method are entirely corroborated by the present study; the tendency to have equal ratios of objective stimuli correspond to equal sensation-differences is strong and natural in such types of sensation as are estimated grossly and from an impressionist point of view, without reducing them to units or conceiving them as thus reduced. We are quite likely to gauge weights by an unanalyzed feeling of effort, which we do not tend to reduce to pounds and ounces, and this is the natural basis of the psychophysic law. We reserve for a future contribution the general discussion of all the results thus far obtained in the application of the method of the psychophysic series to various types of sensation.

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[1] In this case there are reasons for believing that the subject took a very artificial view of the problem before him, and more or less consciously favored the arithmetical series.

Note Upon Other Researches.

To complete the account of the studies of the year, mention may be made of a few studies, as yet incomplete, or to be published elsewhere. In collaboration with Mr. Geo. W. Moorehouse, a new æsthesiometer has been devised, which differs in essential points from those now in use. It permits of testing the sensibility of the skin with a variable pressure upon the points of the skin tested; the motion by which the points are applied is constant, regular and simple. Furthermore, a series of attachments is to be constructed by which the same apparatus may be used for exploiting the various types of tactile sensibility, for the pressure sense, and for the temperature sense; the apparatus will thus test all the chief sensibilities of the skin. The construction is not elaborate and the cost will be moderate.

Two researches of a statistical nature have been undertaken and are nearing completion. One is a study of the dreams of the deaf, with a view of determining the effect of the age of becoming deaf upon the future retention of "dream hearing," and of recording many other peculiarities of the dreams of this class. Mr. E. T. Johnson has had charge of the tabulation of this interesting but troublesome material. The other is a study of association and community of thoughts; the main point being to determine in what degree different persons are apt to think of the same association when starting from a common point, and then following their own line of association. A word is given to a class of students, and at the same time each member of the class writes the first five words suggested by the original word. The proportion of similarity of association in all the first words written, in all the second, etc., as well as in the sum total of all the words is the chief point to be studied; and the main result is the regular decrease of community of association as the words are removed from the original word. The first words suggested to different persons by a given word are more apt to be the same than the second, the second more so than the third, and so on.

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